Anthropology: Ancestors and Heirs

Studies in Anthropology 5

MOUTON PUBLISHERS · THE HAGUE · PARIS · NEW YORK

Anthropology:

Ancestors and Heirs

Edited by

STANLEY DIAMOND

Graduate Faculty,
New School for Social Research
New York

MOUTON PUBLISHERS · THE HAGUE · PARIS · NEW YORK

ISBN: 90-279-7711-9

© 1980, Mouton Publishers, The Hague, The Netherlands

Printed in Hungary

Contents

INTRODUCTION

Anthropological traditions: The participants observed, by Stanley Diamond, Editor 1

PART I: ANTHROPOLOGICAL TRADITIONS

1. Anthropological traditions: Their relationship as a dialectic, by Lawrence Krader 19
2. Anthropological traditions: Their uses and misuses, by Jean Pouillon 35
3. Anthropological traditions: Their definition, by Bob Scholte 53
4. Anthropological traditions: Comparative aspects, by Åke Hultkrantz 89

PART II: NATIONAL TRADITIONS

5. The United States: Racism and progress in the nineteenth century, by Jacob W. Gruber 109
6. Franz Boas: The academic response, by Gene Weltfish 123
7. Germany: Interpreting culture traits, by Paul Leser 149
8. The Soviet Union: The continuity of the great Russian tradition, by Yu. v. Bromley 207
9. Italy: Cultural anthropology reconsidered, by Vinigi L. Grottanelli 221

10. The Netherlands: Structuralism before Lévi-Strauss, by Patrick Edward de Josselin de Jong 243
11. Denmark: All approaches are fruitful, by Johannes Nicolaisen 259
12. India: A Western apprentice, by Surajit Sinha 275
13. Japan: An Asian reflex, by Chie Nakane 283
14. Great Britain: Functionalism at home. A question of theory, by Adam Kuper 293
15. Great Britain: Functionalism abroad. A theory in question, by Jack Stauder 317

PART III: TOWARDS A CRITICAL ANTHROPOLOGY

16. The Manchester School in Africa and Israel: A critique, by T. van Teeffelen 347
17. Latin America: The anthropology of conquest, by Jürgen Golte 377
18. Germany: The search for a new ancestor, by wolf Lepenies 395
19. Yugoslavia: The practical critique of ideological Marxism, by Bogdan Denitch 431
20. The United States: The cultural predicament of the anthropologist, by Eric R. Wolf 455

INTRODUCTION

Anthropological traditions: The participants observed

STANLEY DIAMOND

This book is based on a symposium organized by Dell Hymes and myself under the auspices of the Wenner-Gren Foundation in 1968. Its scope and, hopefully, its depth have been significantly extended since then. Our initial purpose was to bring together a number of anthropologists, representing the various national and/or cultural areas, in order to identify the major scholarly traditions as they were actually put into practice. The questions at issue were set forth in a statement by the editor distributed before the meeting:

> We assume that anthropology, as a discipline, evolved from a common root in Western civilization. Precise areas in space and time may be open to question, but the Western European Enlightenment, particularly in France, and the debating clubs in Glasgow and Edinburgh (the Scottish moral philosophers) seem to have been seminal. In its origins, anthropology was a generalized inquiry, both retrospective and prospective, into the nature and possibilities of man. Therefore, a whole segment of the history of philosophy is directly pertinent to the emergence of the field. As European civilization, and its imperial offshoot, new world society, expanded over the globe, the discipline of anthropology became, on the one hand, increasingly specialized and, on the other, more specifically connected with the various national and/or cultural traditions, both European and non-European. The purpose of our meeting is to explore the following: (1) The historical connection between anthropology as a whole and Western civilization; (2) the particular national and/or cultural traditions or schools that have developed as Western civilization, deploying its techniques, became increasingly dominant and repressive; and (3) the possibility of a unified anthropology based upon the common descent and purposes of a discipline which presumes to be so deeply implicated with the species, its origins and its outcome. The assumption here is that anthropologists can be members of the 'party of humanity' and that the anthropologist should be an engaged

observer. If the latter supposition is open to question, then it is a question which we should consider.

In short our purpose is to reconsider the history, meaning, and possibilities of anthropology.

As it turned out, the three mildly stated intentions of the affair neatly delineate the three sectors of this book, but they did not define the meeting; rather, they overdefined it. We had hoped that a frank exchange would not only clarify the limits of the discipline but would also reveal its potential in the struggle against imperialism and national oppression both at home and abroad. In this and other respects, the symposium, like symposia generally, was both a success and a failure. We succeeded in delineating the major traditions in anthropology, a self-evident conclusion, which I shall undertake to summarize further on. In so doing, we discovered that the notion of a *universal* anthropology was an illusion, perhaps, as Hultkrantz implies in one way, and, as Krader implies in another, also a hope. We learned what we had always known but somehow never really understood, namely, that anthropology was an aspect of the intellectual history of western civilization — at its worst the mortician of that civilization — laying out and presenting victimized cultures to public scrutiny, after the explorer, trader, soldier, missionary and administrator had done their work. But at its most authentic it was the discipline reflecting the self-consciousness of our society in crisis. This is not to claim that western students of man focused on the problems which define the crises or that they systematically conceived their work as symptomatic of the malaise of their own civilization. That is not the intent of institutionalized anthropology, although it did define the consciousness of our eighteenth-century predecessors.

The great majority of modern anthropologists view themselves as academicians and professionals whose positivistic concern is to develop a corpus of cultural theory based on objective reportage, i.e., *the facts*. Typically, they consider it desirable that cultural theory have a non-dialectical, abstract character; man is seen as from a great distance, as the socially programmed or culture-bearing animal. But the relationship between the person and the social environment is not grasped in either a political or normative context, contexts in which men set transcendent goals for themselves and act in order to invent and discover their natures against alienating or oppressive structures. When men behave that way, they do so without the theoretical or practical support

of modern anthropology. Kolakowski's stricture on positivism is pertinent: 'Suffering, death, ideological conflict, social classes, antithetical values of any kind – all are declared out of bounds – positivism so understood is an act of escape from commitments, an escape masked as a definition of knowledge.' I should add that anthropologists may perceive their institutionalized discipline as a phenomenon of cultural history in a sociology-of-knowledge framework (e.g., de Jong), but they rarely subject their *theories* to the same critical scrutiny (see Gruber). Anthropologists are most comfortable when they can conceptualize their findings as ultimate, as supposedly scientific regularities. Presumably, these emerge in history as do the 'laws' of physics in the mind of the academician, but once having arrived, they become permanent. Retrospectively, it is easy enough to give history the appearance of immutability, but in time the question of praxis, the dialectical question, intrudes; the self-reinforcing character of academic generalizations is thrown into relief. They comprise part of what Marx called the fetishized consciousness which underlies all ideological formulations; that consciousness is, ironically, clearly evident in the study of man.

Anthropology originally had a critical and dialectical potential, centered on action in the world, quite obvious in the eighteenth century. But its professionalization since the end of the succeeding century sealed anthropologists off from the ancestral promise. As Gruber points out, nineteenth-century anthropological* theory in the United States rationalized the growth of social inequality and internal colonialism. Moreover, the notion that anthropology had as its central purpose the further understanding of our own civilization had lost strength as the discipline became increasingly implicated with colonialism, imperialism, and, in the twentieth century, noncritical relativistic rationalizations about the equal dignity, equivalent structure and/or functions of *all* societies.

The symposium failed, then, in achieving a useful dialogue on the issue of a critical, and self-critical, anthropology. With the exception of Wolf's paper, the last section of this book is based on contributions that were invited in succeeding years. Hence, the interesting juxtaposition of papers on British functionalism, German ethnology and Soviet and Yugoslavian anthropology. However, it must be pointed out that critical anthropology has only recently been revived as a serious undertaking among scholars who have enough faith in their personal integrity

* In contrast with certain ambiguous aspects of ethnological theory.

to take political action, deny the 'objectivity' of the discipline, understand its involvement in imperialism and refuse to accept its dissociation from domestic crises. But the fact remains that our expectations concerning critical anthropology were premature. I have stated that anthropology as an aspect of the intellectual history of western civilization was extraordinarily self-conscious. I think that this remains true, even apart from the old/new tradition of critical anthropology, if we define that self-consciousness as alienated. Put another way, the self-consciousness of the professional anthropologist was expressed in his sense of marginality. He was perpetually commuting between two or more cultures, physically and/or intellectually, even if his primary concern was in the deep historical background of his own society. The alienation of the anthropologist, of which he is inexplicitly conscious, along with his compulsive search for cultural alternatives, signifies his awareness of the civilizational crises that he rarely has either the tools or the impulse to analyze, much less act upon, except in the most obvious and conventional manner. Alternatively, a few anthropologists take evasive action by speaking out strangely and portentously on the human condition in general.

For the profession of anthropology *can* be used as a refuge, a means of evading the engagement of the person in his own intolerable social reality by substituting for it what then becomes, because it is a mere substitution, the *mock* engagement of the field experience both at home and abroad. If most conventional scholars turn the academy into a retreat from the world at large, the anthropologist tries to turn the world at large into an academy. This is not to deny the dedication and personal risk which can be found to one degree or another in the careers of most anthropologists. But these are, typically, alienated risks of a curiously egocentric character which, in turn, justify the anthropologist's self-definition as a person holding himself apart from civilization – perhaps as a critic but, if so, a universal critic and, therefore, not a critic at all. Being universally critical is simply the obverse of being universally tolerant; anthropologists (sometimes the same anthropologist) adopt both stances, at the expense of being either discriminating or convincing.

Predictably enough, the symposium did not spontaneously generate a discussion of problems of that order. The very presence of scholars more or less connected with the official or quasi-official academic and research institutions in their native lands inhibited open exchange.

Conceivably, Englishmen might discuss among themselves, unofficially at home or in their clubs the link between anthropology and imperialism, but they are unlikely to do so in the presence of students from other nations. Possibly, Russians might find the domestic opportunity to elaborate on the national question or on a Marxist critique of bureaucracy, but they were hardly about to do so in the presence of Americans or Englishmen.

The point is that an international symposium is the least likely place for a consideration of sensitive issues by sensitive men, attended as it is by scholars conventionally distinguished enough to receive invitations and who behave as courteously treated guests in another country. Under such circumstances, it is the most abstract questions, on the one hand, and concrete information of a routine character, on the other, that command attention. On these public occasions, anthropologists comport themselves like intellectual diplomats, a skill that has been sharpened by their years of participant observation in the field and which, when practiced for solely professional ends, hardens empathy into mere acting. Still, one has the sense that if these accomplished men and women had spoken to one another without impediment, they could have said enough to subvert a dozen official ideologies. Anthropologists generally, I think, know more than what they can formulate; their alienation is continually reinforced by the very professional practice which catered to it in the first instance, and they do not, therefore, ordinarily penetrate very far into the ground of their own behavior.

Yet the symposium also succeeded. To begin with, we learned a good deal about ourselves, or, at least, we were given the opportunity to become more acutely conscious of how we behave. Some of the instruction was inadvertent: Professor Nakane, for example, tells us (the details are unique, the circumstance is familiar) that following the invasion of the continent, the Institute of Ethnology, set up by the Japanese Ministry of Education, sent scholars to North China, Manchuria and Mongolia to undertake field surveys. 'Unfortunately', these surveys were aborted (as they were, also, in Formosa) since 'the war turned out badly for the Japanese'.

Professor De Jong states flatly that 'cultural anthropology in Holland [started] its career as a university discipline with the purpose of giving practically useful knowledge of Indonesian people to prospective colonial administrators'.

Professor Grotanelli alludes to the 'tragic events of the [Second

World] War [which] not only brought all scientific research to a standstill but acted negatively on Italian anthropology in a psychological sense by discrediting the utility of studies that rightly or wrongly the general public took to be connected with colonial policies and by discouraging incipient vocations'.

Professor Nicholaissen relates that the ethnographic museum in Copenhagen, the first of its kind, was established in 1841 in the context of the 'small Danish empire'.

Professor Fortes (who is not represented in this book) frankly notes that he was grateful for the colonial framework in which he undertook to understand the life of the Tallensi, although this was admittedly of limited value to the Tallensi.

Professor Huitkrantz assures us that ethnology is '...*not* the hobby of the privileged minority in a class structured society, [that view] is a regrettable confusion of the amateur explorer [with] the academically trained field researcher and theorist'.

These honest observations are, of course, commonplace, given the ambiguity of the origins of the discipline, but the point is that the anthropologist himself does not often confront, though he is aware of, that ambiguity. George Grant MacCurdy, a pioneer of the American discipline, makes the case for us unwittingly and in some detail. In an article in 1902 in *Science* on the growth of Section H, the anthropology branch of the American Association for the Advancement of Science, he wrote:

> Patriotism is a more or less constant factor in inspiring one with a love for everything pertaining to the home-land; archeology and ethnology, as well as form of government and commercial, artistic or literary supremacy. We cherish some relic of a vanished race all the more because it was found on the old homestead. Local, national, New World pride has evidently had much to do with our choice of subjects for special research. Add to all these considerations a vast and virgin continent awaiting the anthropologist and there is little wonder he has given such a relatively small portion of his time to the Old World, or the islands of the sea.
>
> ..... [But] The cosmopolitan character of the programs of the several (Foreign) associations in question is found to be in direct ratio, not only to the area of the colonies and dependencies of the several countries, but also to the tonnage of their merchant marine engaged especially in the foreign trade. The anthropologist's horizon is constantly under limitations imposed by his government's colonial or commercial policy.
>
> With colonies and protectorates beyond the confines of Europe aggregating over 11,000,000 square miles in extent, including India, and with a merchant marine engaged exclusively in the foreign trade, much larger than that of any other country (8,043,860 tons in 1899), open especially

to them, the English anthropologists are brought into contact with foreign problems at so many points, it would be strange indeed did they not improve the opportunities thus afforded.

The colonies and dependencies of France cover an area (1901) of 3,740,000 square miles, with a population of 56,000,000. The area of German colonies and dependencies amounts to 1,027,120 square miles with a population of 14,687,000.

The United States became a 'world power' only three years ago. Enough time has not elapsed to show the influence of that step on the programs of Section H, but if we expand along with our opportunities, it is safe to say that an analysis of the work we shall do in the next twenty years will show different results from that of our record for the epoch just closed.

We may not be able to improve much on the quality or even the quantity, but, with an enlarged horizon the work should become less and less local and fragmentary. I believe we are at the threshold of a new epoch in which the many interdependent and partially solved problems of the past shall be completed and thereby make possible vast progress in correlative and synthetic anthropology.

It is, then, the growth of the science, not the 'subject' people (subject in both the experimental and political senses of the word), that remains the anthropologist's paramount concern. Wars, revolutions, political movements – these are incidental, if inevitable – they do not dissuade him from the pursuit of his objective goals which, at best, may be said to be pursued for their own sake, whether that is acknowledged or not. Nor do anthropologists willingly admit to distortion of perspective because of the colonial setting in which they are introduced to the peoples they 'study'. As Dr. E. N. Obiechina has stated in his introduction to an edited volume on *Igbo traditional life, culture, and literature,* (*Conch Magazine*, September 1971), the contributors to which are themselves of Ibgo origin, 'hired anthropologists, administrative anthropologists, and zealously proselytizing Christian missionaries are hardly the kind of people [to understand] a subject people with a widely differing way of life'. And he and his colleagues go on to analyze why it is almost impossible for the academician, trained in Western categories, to grasp the most subtle and significant elements in Igbo traditional culture. One should add that anthropologists are usually reductionists or idealists – as opposed to dialecticions – and assimilate the highly sophisticated cultural expressions which they encounter to adaptive, reflexive or functional phenomena. That is to say, they sociologize, psychologize or economize them out of existence, which mirrors, of course, the imperative of their own exploitative and conflict-ridden societies.

Anthropologists have entrenched themselves in their privileged posi-

tions not out of a desire to exploit but because they have forfeited any alternative in selecting the alienated profession as an ecological niche in the first place. It is an adaptation they will not lightly question, for they are not merely academics but also intellectuals of a peculiar disposition, one that perhaps is peculiar to modern Western civilization and most symbolic of its alienating character. The paradox is that these students of culture, who seem so eager to leave their own, are nevertheless frozen in its grip and implicitly express its deepest and most catastrophic assumption, namely, that man can be thoroughly objectified by man. Therefore, they are not dismayed by the likelihood that the professional anthropologist can no more know the native than any master knows his slave. Rather, he can know only the response of the native to him and what he himself sees of the native culture (see, for example, Pouillon).

This would not be so bad if the questions the anthropologist asked were as searching as those anticipated in the eighteenth century. But, as Eric Wolf also concludes, the questions have become sectarian, 'professional' and trivial. One can readily imagine that the corpus of anthropological work, with some minor exceptions, will in some future, more clear-sighted time, be perceived as little more than a reflection of the attitudes and the intellectual play of an imperial civilization. And the record will reveal, I think, that anthropologists had only the most abstract interest in the cultures they studied.

That is why Paul Radin – at one with Boas – insisted that only the native could reflect his culture and that – here he was unique – only the ensemble of these individual reflections could be defined as history. The role of the ethnologist was, therefore, that of a midwife, and all European interpretations of indigenous cultures were false if they were not inherent in the narratives of native individuals. (This also seems to be Clifford Geertz's meaning when he refers to reading such a culture as one would a literary text.) The relentless honesty of this view can hardly be denied, for what Radin meant is that, otherwise and typically, anthropology presents the European conception of the 'past' and does so for Europe's sake. The purpose might be comparative, illuminating, for example, some aspect of modern civilization; characteristically, however, the function (if not the conscious purpose) of anthropology has been to rationalize the destruction of other cultures in the name of progress as defined by and diffused from a few imperial centers. Radin's view relieves us of the illusion that we have somehow incorporated the

cultural possibilities of the past in the prevailing definition of the present. For how can we attain that if we fail to understand what has been destroyed?

Krader confronts this issue when he imagines an Eskimo concept of man projected 'in the plurality of peoples, and the concept of man in nature' – which would, nonetheless 'be advanced for ... its ... own ends ... and thereby our common end'. He does not imagine that the Eskimo view, as he defines it, can be useful to Western science because it had already been negated in the Western tradition as part of what has been judged to be fallacious in Aristotle. But Krader confines his observation to a supposed theoretical similarity between Aristotelian and Eskimo methods of classification. He does not refer to Eskimo *technics* which have proved essential to Europeans occupying the Arctic. On the other hand, Krader admits to no ground for rejecting Eskimo culture as a whole, although he does not mention the problem (of which he is certainly aware) of Eskimo self-determination and development on their own terms. The latter poses a political question of such acute importance that it demands the redefinition of politics in cultural terms if Western civilization is to survive its own destructive 'universality'.

It is, of course, comforting to adopt a deterministic theory of history in order to satisfy the 'scientific' conscience, while evading the possibilities of cultural growth on bases other than those predicated in theories of 'modernization' imposed by imperialists on natives. Thus, it is only in the abstract that anthropologists have celebrated cultural diversity; in reality, they have functioned as agents of their dominant societies, and they have been among the first to decry as romantic the effort to deny change other than in the image of their own cultures. The unexpected vitality of national movements, which are, essentially, demands for the perpetuation of cultural diversity in politically viable form on an economically viable basis, has not caught the fancy of anthropologists, who have long since accepted the 'realities' of centralization and bureaucratization as the road to what is called 'progress', 'international amity' and so on. Even the most orthodox ethnologists, while paying lip service to the notion of ethnogenesis, tacitly agreed that they were studying the *past*. Whether or not they were evolutionists and/or progressivists in the ethnocentric, or more broadly philosophical meaning of the terms, they accepted (and continue to accept) the constriction of cultural alternatives in modern society as inevitable.

This is all the more understandable if we recognize that the major traditions of professional anthropology have been formulated in metropolitan Euro-American civilization. I do not refer here to the concern with 'folk life' which, throughout Europe, dates back to the very formation of the peoples involved. Work of this sort can, for example, be traced to the seventh century A. D. in what was to emerge as 'Russia', as Bromlay points out. The study of folk life can be viewed as the transformation of an oral into a more or less interpretive written tradition. The folk life of a people as reflected in folkloristic work may, of course, lend itself to the uses of chauvinistic nationalism and other political perversions. But more profoundly, folk studies seek to reflect the historical dimension of cultural meanings in both their continuity and discontinuity. This seems evident in the work of Sigurd Erixon and is generally the view of the circle connected with *Ethnologia Europaea*, in contrast to the more mechanical, scientistic and 'universal' consciousness that dominates *Current anthropology,* to which Hultkrantz alludes. The Hungarian ethnologist Ernő Tarkany-Szűcs has, in the essay 'Results and task of legal ethnology in Europe', published in 1967 in *Ethnologia Europaea,* clearly reflected the folkloristic consciousness of this recently organized group.

Folkloristic studies, then, are symptomatic of the *problem* of cultural diversity in the modern world as perceived by folklorists themselves. And they become a repository for the preservation of cultural singularity, which turns out to be one of the rationalizations (not the achievement) of modern ethnological study of exotic cultures, but with this critical difference: folklorists are typically interested in the possibility of identifying, reviving, maintaining or extending the folk life which they study. For them, folklore as a discipline and folk life as an actuality are mutually reinforcing. Therefore, the folklorist shares a certain understanding with the colonized native: Both are aware of cultural decline. But the folklorist is precisely the kind of native ethnographer of his own culture that has not been created in 'aboriginal' cultures, for the folklorists' society, in the course of becoming a modern imperium, transforms itself, losing its own past, its own particularity, which the folklorist works to recover, but the past of the other, the imperialized society is reduced to an abstraction in the mind of the conquerer. It follows that the student of folk life shares, to a certain extent, with the 'exotic' novelist or poet, the perception that the indigenous culture of each has been destroyed by processes familiar in the modern world; he and they preceive 'mod-

ernism' as a problem. Thus, he is likely to believe that the circumstances for the very creation of culture are being rapidly dissipated in the bureaucratic structures which characterize and simultaneously radiate from the imperialist centers. The folklorist has the ethnological consciousness, but he is not a professional ethnologist. He does not accept the past as dead; rather, the death of the past is his dilemma.

Although a few anthropologists share the folkloristic sense, professional anthropology is not based on the study of folk life, nor is it any longer grounded in its eighteenth-century origins. It must now be understood in terms of the functions of a limited number of intellectual traditions, conceptual modes or academic perspectives that have no bearing on the actuality or possibility of cultural persistence and diversity. These traditions, which have been specifically delineated in the essays in the second section of this book, developed in Western Europe only within the past century. If one were to draw a somewhat distorted triangle connecting London, Berlin and New York, it would just about encompass the area of origin. These inter-fertile traditions were diffused in various permutations and combinations to academic centers throughout Europe, Asia, Africa and Latin America. They have coalesced into what should really be defined as a single tradition with a number of aspects or complementary perspectives. This is evident, for example, in Professor Nicolaisson's reflections on his own catholic theoretical preferences.

Professors Peristiany, Grottanelli, Nakane, Sinha and Leser readily acknowledge that contemporary anthropology, no matter where practiced, is a reflex of the major traditions. In this sense the discipline is about as universal as the Singer sewing machine. It is a diffused technic that can even be fabricated abroad. 'International' anthropology has not been built up inductively as a result of the actual concern with cultural tradition and reflection on culture, in the various national and/or cultural areas. Rather, it has become a uniform profession, put to the same uses in Bombay as in London, Tokyo, New York or Moscow. Professional anthropology is an instance of diffusion by domination; it represents a single, historical case and, therefore, cannot be proved to be the necessary result of the evolution of the 'study of man' or a synthesis of universal knowledge about human beings.

An Indian or African anthropologist, trained in this Western technic, does not behave as an Indian or African when he behaves as an anthropologist. He may be a structural-functionalist, an evolutionist, a

cultural historian or an eclectic combination of these, and he lives and thinks as an academic European. When we claim, as has frequently been done, that it would be revealing for an Indian anthropologist to study a community in, say, Indiana, we are deliberately missing the point. For such an anthropologist would apply his scientific training in order to produce 'verifiable' results in the anthropological canon he has been led to adopt, which, in turn, is accommodated insofar as possible to the uniform Western definition of science, which cross-cuts all forms of systematic inquiry. We cannot, therefore, turn to anthropologists of another national origin in order to illuminate our cultural existence – or even to identify the 'culture of our culture', that which transcends our specific definable culture and touches the universal in the cultures of others. That task falls to the social critic, our own artists or, more emphatically, writers and poets growing out of other cultural traditions who, in understanding the assault of the West on their people, come to understand those things about ourselves which we would otherwise hardly perceive. On the other hand, unless we learn to ask significant questions of other cultures (not just novel or clever questions, but those vital to our civilization, illuminating our deficiencies), anthropologists will not be able to interpret their own, since criticism proceeds by contrast, and their sense of human possibilities will progressively diminish.

Thus, professional anthropology is caught in a double bind: If the past is dead, if *we* are the present inheriting the past, if we are the surviving human possibility, how can we understand other possibilities and how can we know what to think about ourselves? Are we obliged to consider ourselves man's fate? But this dilemma is, unfortunately, if ironically, confined to anthropologists and other members of the academic, and thus social, establishment. The anthropological double bind, one hastens to add, is not necessarily a problem for artists, advanced political thinkers or 'ordinary' people looking for solutions to the dilemma of 'modernism' (see, e.g., Henri Lefebvre in *Revue Francais de Sociologie* No. I, 1960). The paradox is that anthropologists who presume to such varied and critical knowledge, by virtue of their method of distanciation, the last stage of relativism, now find themselves isolated in their own society, class and occupation, and what they know or how they formulate what they know is useful only to the powers that be.

If there are any recent exceptions to this state of affairs, they are

the exceptions that prove the rule. The current emergence of a critical anthropology, drawing on the best intentions and results of the past, is a response to the entrapment, a result of the hyper-professionalization and hardening of anthropological traditions, in academic departments in the centers of imperialism. The contemporary study of man was grounded in the transnational motifs of the Western European Enlightenment and now, after two centuries of regional differentiation in the context of the colonial roles and metropolitan growth of the European powers, we encounter a new, but a bureaucratic, uniformity. This, of course, must be distinguished from the open-mindedness, the universalism of the eighteenth century. It is, rather, the reification of the eighteenth-century vision, reflecting the supranational capitalist state. Therefore, for reasons of both its origin and destiny, it becomes evident, as soon as we attempt to isolate any given anthropological tradition within a particular cultural or national area, that we are dealing with complementary or overlapping perspectives.

In the United States, for example, Franz Boas established academic anthropology as the integrated study of man, with each subdiscipline (linguistics, cultural anthropology, pre-historic archeology, human biology) corresponding to a critical aspect of human behavior. Boas did not invent this four-fold division; it corresponds quite logically to the major questions that one would ask if one were trying objectively, in a scientific context, to understand a form of apparently intelligent life on another planet, namely, do they communicate symbolically, and how? What activities define their social life, and how are they related? What is the material evidence for their social and speciational history? How do they compare with super-primates such as ourselves? Rousseau had, in the *Second discourse on the origins of inequality among men,* already understood the inevitability of these interrelated questions in setting forth his model of primitive society and its transformation. The four-fold division of the field has, then, a logical and a historical basis.

To pursue the New World convention, which nicely reveals the interrelatedness, if not the denouement, of the orthodox canons, one notes that Boas was born in Germany, studied physical geography and had been deeply influenced by the explorations of German Kulturwissenschaft – in particular, cultural history, cultural geography and cultural psychology – which demanded a wide-ranging knowledge of man. There is, therefore, a generic connection between the German and American

traditions; when Boas migrated to the United States toward the end of the nineteenth centry, he brought with him the best potential of German anthropology.

But in the United States, there is also a subsidiary tradition which may be called incipiently dialectic, evolutionary and progressivist, in the more positive sense of that term, centered on the work of L. H. Morgan. Morgan's theoretical heritage, which was slightly anterior to that of Boas and never grounded itself in the Academy, did not flourish in his native country, although his intense interest in the fate and structure of Amer-Indian society did. It served, rather, to complement, and was to a certain degree the basis for, the ethnological perspective developed by Marx and Engels, and it therefore became the manifest root of Soviet ethnological theory, if not of Soviet ethnological *praxis*. At the same time, Morgan's more technical, paradigmatic effort in kinship studies served as the ground for English and, to a certain extent, French 'structuralist' concerns, as they have been called, but the term is historically out of place; systematic focus on kinship, or kinship as a system of social relations, is a more accurate characterization. In the United States, however, Morgan, the property-oriented evolutionist, eventually found a self-proclaimed contemporary heir in Leslie White, and White has been the major source of the nondialectical, mechanical materialism adopted by a number of younger academic anthropologists, in turn traceable to the positivistic potential of eighteenth-century thought. But the explicitly Marxist implications of Morgan's work, that is, its Marxist intentionality as conceived by Engels, has not been seriously pursued.

Morgan's fate in France is proving to be rather different, since Emmanuel Terray, among others, has now interpreted him within the framework of an emerging dialectical and critical, a Marxist, ethnology, which is, quite predictably, opposing itself to structualism. In France, the major tradition has, of course, been structualism evolving out of the well-known French sociological school. Nevertheless, Lévi-Strauss, the ultimate representative of this approach, has acknowledged his indebtedness to American empirical ethnology, whereas the representatives of British social anthropology have linked themselves to both structualism and Durkheim. These identifications may be subtly misconceived, or even self-serving, but the affiliation is, generally speaking, sound enough. To complicate the geneology further, the particular structuralist conceptions of Lévi-Strauss were anticipated by the Leiden school of Dutch anthropologists (in turn rooted in Durkheim and Mauss)

who had worked primarily in Indonesia prior to the Second World War.

Social anthropology, that is, structural functionalism, was, of course, the major tradition, but the important subsidiary effort in England was in the cultural and evolutionary perpectives of Tylor. Tylor's teachers, in their turn, came out of the Scottish variation on the Western European, specifically the French, Enlightenment. His classic, incorporative definition of culture, however, did not find a congenial place in the British political academy, alert to the need for insights providing social and administrative leverage; his theoretically ambiguous, sometimes self-critical evolutionism could not be readily assimilated to the colonial context, in which nonhistorical, but instrumentally oriented, functionalist field studies prospered. For reasons of both State and Theory, the British functionalists found it expedient to conceive society in frozen cross-sections. On the other hand, Tylor's global view and definition of culture became common currency in the Boasian academy, while his evolutionism was linked to Morgan's and woven into the subsidiary tradition of American anthropology. There is a sense in which Tylor, representing both aspects of the American tradition, could be considered the paridigmatic anthropologist of the New World. His journey to Mexico is strangely a propos; the distinction is that he was not a professional in the Boasian sense, and of course he remained an Englishman.

One could go on identifying the various permutations that have occured in time and space as a result of the underlying conceptual uniformity, diffusion, independent development and convergence of the major traditions. But these are more than adequately put forward in the essays in this book. It is, I think, sufficient to indicate that British social anthropology, French structualism and American general anthropology, along with the salient subsidiary tradition Anglo-American-Russian evolutionism (in the last case, it can be called positivistic Marxism), have all become frozen in their tracks and reflect the ethnocentrism of their respective establishments. One can hardly refer to a German anthropology of any consequence after Boas (excepting the species of cultural-historical interpretation admirably explicated by Professor Leser); it simply disappeared into the vortex of the Second World War, leaving behind a basically unchallenged Fascist ideology, it self a deformation of ethnology, at least half a century in the making, but I cannot pursue that unexamined aspect of German intellectual history here.

The limitations of the major traditions are symptomatic of the respective domestic predicaments of the intelligentsia, linked to the position of their societies in the world. Despite the nuances that distinguish one tradition from another, what they now share, notwithstanding frequent disclaimers, are nondialectical, progressivist and evolutionist assumptions, interpreted in the interests and in accordance with the values, the highest values, of course, of the metropole.

Therefore, I conclude that a critical anthropology, conscious of its origins and limitations, is obliged to build itself outside of the academic boundaries that have constricted the eighteenth-century potential. But it is not enough to reject the academy; a discriminating rejection of the social reality which the academy reflects is also necessary. (It is germane to recall that neither Rousseau nor Marx ever occupied positions at a university.) Put another way, one must recognize that theory and practice cannot be dissociated from each other in the reconstitution of a critical anthropology. If the present theoretical state of the discipline is linked to the academic practice of its proponents, then to change that state means to change the practice. It is not a question of anthropology becoming political but rather of anthropology becoming conscious of and changing its politics. It will either become the partisan of the people it has hitherto studied – at home and abroad – and help recreate a society responsive to human possibilities, or it will become nothing at all. Yet, if academic anthropology must become a casualty in the international struggle for cultural diversity and socioeconomic equality, it will, ironically, have yielded to, or in another sense fulfilled, the 'object' of its study. And the tradition of Rousseau and Marx, the ground for critical anthropology, will have come full circle.

PART I

Anthropological traditions

1 Anthropological traditions: Their relationship as a dialectic

LAWRENCE KRADER

1. The unity and plurality of anthropological traditions

The questions that we will take up in this essay are: In what does our anthropological tradition consist? Whose tradition is it? What are the limitations of the tradition? We will also consider how that tradition has been and can be advanced. We start from the premise that our tradition, in both its theory and its acts, is a cultural phenomenon, necessarily one of our own culture. The positing of an anthropological tradition is at once a positive and a negative act. The positive of the concept is the processual, the handing on of the notion of anthropology as the science of man rather than as a state of being, an origin, a resting point or the terminus of the process. The negative of the concept is the juxtaposition and opposition of the traditions which together comprise the tradition.

There is no doubt that an anthropological tradition exists. But there is some doubt among anthropologists that this tradition both exists as the component of a culture history and arises as a function of that culture. By virtue of this doubt, the particularity of the anthropological tradition is denied, and its pretension to a pan-human ambitus is affirmed. If we regard anthropology as both the product and a part of a given tradition, then on what basis is its warrant for universality founded? Our assertion of anthropologist cultural-rootedness constitutes at once a limitation of that warrant and the sole means of its realization. The limitation of our anthropology arises from the premise that we perceive, think and feel simultaneously in the universal way and in the particular way of our own culture. The warrant for universality consists in the

critical parsing out of what is the universal and what is the particular in our thought. As such, a universal anthropology exists as a potentiality of our tradition, for the critique is not at an end, and the combination with similar mental activities in other cultural traditions is only now beginning.

Histories of anthropology generally trace the course of our discipline from Herodotus, Hippocrates, Aristotle, Strabo, Pausanias, Ptolemy and so forth. They are written with the canons of ethnography in mind, as found in the writings of those who first described the varieties of customs and the place of man in nature. The minor theme is the development of the concepts and institutions of our discipline – the culture, the collectivity, the person, the individuum as he becomes a person. By our critique, we free the concepts from their terminological fixity in one cultural tradition so that they may be applicable to all. The end product of this exploration is a methodological precision in the adoption of our concepts. The potential of a pan-human science of anthropology lies, to begin with, in the critique of our usages, for our discipline comprises the critical study of ourselves as men of a given culture and professional men of that culture, just as it is potentially the product of the study of all cultures and is the confluence of their histories.

The idea of culture has been made into a canon of orthodoxy in recent times. As cultural anthropologists, we note that the Latin term with its derivatives 'cultivate', 'colony', etc., is the conceptual product of a farming civilization, with its child-rearing, educational, economic, political and religious practices. The focus of the concept, not in its earlier forms but in the way we have pared it down in our own professional use, is the cultivation of the individual, whether human, product or collectivity. The Eskimo or Chukchi concept of man, that is, of our identity and differences among ourselves and of our identity and differences from other natural beings, is unlike ours at the present time, but it resembles that earlier in our tradition. We have devoted much effort as a discipline to the perfection of our concept, first within the sphere of all humanity and then in narrowing it down to our own cultural sphere. What would be the result if an Eskimo or Western ethnologist were to perfect (for by now we have the potentiality to do so) the Eskimo concept of man in the plurality of peoples and the concept of man in nature? We would have to accept that the perfection of the Eskimo concept would not be advanced for our own ends as scientists of Western civilization but for its and their own ends and thereby our common

end. The eventual combination of the way of others and ourselves as a synthesis of the differences is a problem that can only be resolved dialectically. Such a view, while it is optimistic and indeed progressivist, is not necessarily culture-bound.

The Eskimo concept of man is static, contrasting states of being human and nonhuman, without passage from one to the other. As did the Aristotelian, the Eskimo conception argues for fixity of species, recognizing likeness in man and other beings but without positing a developmental process. The Chukchi cosmology has attributed human qualities of knavery and foolery to animals, anthropomorphizing the trickster Raven and humanizing the Creator, who is married, gives laws and clothes himself against the cold. The Eskimo and the Chukchi conception of man is already known, changing certain details, in the tradition of Western anthropology, where it has been superseded by developments in biology and anthropology in the nineteenth century. Our criticism of the past of our own intellectual tradition includes the criticism of the Eskimo, which in this connection is of interest as a variety of human regard of himself, but for us and for them a by-passed interest. The extreme of cultural relativism which has been proposed within our discipline is subject to our critique. But in order to do this we must first establish a common ground between a trend of thought in our own culture and one in the other, taking both seriously as having importance. Positively, we cannot dictate what others are to think, but the standards of error, negatively, applied in the one case may be applied in the other.

Feng Yu-lan, Marcel Granet and Joseph Needham have contrasted the Chinese philosophy to that of the Western, without specifying the elements of a Chinese anthropology that are different from our own. But it is clear from the writings of Granet and Needham that the Chinese philosophy of organism developed the concept of man in a way quite different from that of the West, which is in the main the heir of the seventeenth-century mechanistic and dualist philosophy and which has had an impact in recent times on Chinese thought. Max Pohlenz has been able to identify in the Stoic Posidonius the theme of the world as organism. L. von Bertalanffy was able to read a development of the theme in Nicolas of Cusa, once the latter's thought was stripped of its mysticism. Needham himself traces the theme to Leibniz, although he there loses himself in the speculation of a Chinese origin to Leibniz' conception of the theme. It is further developed in Hegel's concept of

Geist, which was consciously developed from its Stoic root. At the same time, Hegel used the concepts of organism and inner organization synonymously, a notion that was picked up by Marx in his critique of Hegel's philosophy of right and law. It is echoed obscurantistically in Bergson's idea of organic memory and enters into the formal anthropological tradition through Frobenius by way of Lotze, with the separation in the former of culture from society. Again, it is traced through Comte's idea of the Great Being, Humanity, to Spencer's superorganic. Kroeber took up the notion from Spencer and Frobenius, and at the same time from C. L. Morgan's and S. A. Alexander's philosophy of culture as evolutionary emergence. The idea of organism as a monistic theory of nature and of man's place in nature has been further developed by A. N. Whitehead, Kurt Goldstein, Needham and Lewis Mumford.

Again, the Hegelian idea has come down through another line, through Otto Gierke's idea of the *Genossenschaft,* literally societas and by extension the communal association, and Wilhelm Wundt's *Volksgeist,* to Ferdinand Tönnies, and thence to contemporary discussion of community, collectivity and the psychic unity of the collectivity. However, whereas the organic is a major theme in traditional Chinese thought, it is a minor theme in the West where it is just now being made operational scientifically, with apparatus for testable hypotheses, observations of growth series, alternatively of group unity and the formulation of scientific laws. Organic theory, with its implication of differential rates of emergence, formation and development, is relevant to evolutionary theory which, as expressed by most of its proponents in anthropology, has not been in close touch with related areas of biology in recent generations. We shall return to this point in regard to evolution and history as well.

Now, if we ask what warrant Western anthropology has for its claim to be a universal science of man, the answer lies, perhaps, in the consideration that a given of our anthropological method is that we are the creatures of a particular cultural tradition and that our thought is determined within that tradition, being one of its determinants in turn. That tradition, in all likelihood, actually embraces all or most of the conceptions of man as a simple and as a complex organism, of human variety and of man's place in nature, as expressed in other cultural traditions. Our own tradition has provided a number of incentive factors in current anthropological thought: A great time depth, a great variety

within it and an increasing degree of self-consciousness. The given intellectual tradition is a component of a civilization of great force which early conquered and exploited peoples, and regulated, in its institutions, the relations between conquerors and conquered, incorporating their ideas as well as their material goods. In the times of classical antiquity, not only were ethnographic data amassed, but general laws of human development were also formulated by Aristotle in *Politics* and *Ethics,* by Lucretius in *On the nature of things* and by Justinian in *Corpus Juris Civilis.* Since that time we have perfected their observational techniques for human societies and their ideas of cultural movement. We have made important modifications in them to take in, and take into account, the great accretions of materials since the age of discovery and world conquest (to call things by their proper names), with further modifications since the formation of the discipline of anthropology as a profession in the last three generations.

The warrant for universality of our anthropological formulations, however rich they may be, has not been adequately tested. First, the consciousness of the particularity of our tradition as the matrix out of which the anthropological doctrines have emerged is a general matter, accepted in principle but not developed in detail. The implications of this are only in part that we do not fully know our own history. We have already alluded to the fact that we know the history of our discipline in a certain way, have recognized the early ethnographic accounts for what they are, but we do not know as well the history of our concepts so that they may be traced in all their imperfections and changes of name in their development. We have not determined the concepts of culture, collective, structure and person *ab ovo* but have perfected them and turned them to our use. Second, we need an inventory of similar concepts arising in traditions other than our own which have considered the nature of man, his ethnic varieties and his place in nature. We may start with learned traditions such as the Chinese, Japanese and Indian, performing tasks for the science of man such as Needham has done for the natural sciences in China. In compiling such an inventory, we would be guided by a law of diminishing returns which would decree at what point the further amassing of concepts was no longer worth the efforts of compilation. We may then satisfy ourselves, perhaps, that we really have got close enough to closure in our lexicon of basic concepts in anthropology and are not in fact limited by our own tradition. A lexicon of basic concepts would be conceived not as a canon but as a point of

departure. Unless it were conceived as a starting point for critique, such a lexicon would be the death of a discipline which is empirical and not deductive. It is conceived as self-effacing, or the discipline as self-destructive.

The concept, where it is found in each tradition, is returned to the others and is thereby posited as a universal in its potentiality. Having been advanced in terms of one tradition, the concept in each case will be passed into the others if it is not already found there, and by their critique returned to the unity of the subject of anthropology. The passage from the Western tradition of anthropology in its unity to the plurality of traditions and the return to the unity as a potentiality is in this way directly subordinated to the dialectical resolution noted above as its passage from the abstract to the concrete. A universal anthropology is to be established by the step-by-step combination of the concepts in the different human traditions, of which ours is the most forceful but not the only one. The forcefulness and ambitus of the one tradition is a snare which by its success fosters the illusion of universality and totality, namely, that its formulations are applicable to all mankind and that all human concepts are already comprised in our tradition. The Renaissance thinkers, on the eve of world dominion by occidental man, were more modest in their ideology. Prophetically, they expressed their willingness to accept everything human unto themselves. We have escalated their intentions as the means of conquest which they innovated have been totally developed and exploited.

A third stricture raised against our warrant for anthropological universality is a qualitative one. It is clear that even though the organic idea is present in Chinese and Western thought, and possibly elsewhere as well, it is differentiated by nuances both subtle and powerful, only one of which is the relative weight of the idea in the Chinese and European traditions. The differences, further, are semantic, epistemological and ontological. The exploration of the nuances is important as revealing both how the ideas are expressed and how cultural variations on a common theme are composed. This is the stuff of human thought, and we can gain an insight into our warrant for universality both by study of the variations and the affirmation of their presence or absence in a given tradition. At the same time we can gain a critical insight into our own thinking by noting the nature of our variation upon that which is common and the broader influences that called forth the given variation in our own tradition and in the other.

The critique of the cultural limitation within which anthropological theory has been developed has a bearing not only on the cultural-rootedness of the theory and on the systemic generation of culturally particular theories but also on the making conscious of its cultural matter, namely, our ideas. In this way the critique passes into the domain of practice, for it thereby effects a change in the cultural matter, for instance, in science education. The potency of anthropology is that only by its practice may man exceed his cultural tradition.

2. Critique of the opposition of subjectivity and objectivity

Marcel Granet, according to the report of Marcel Mauss, noted that the idea of a person is absent from traditional Chinese thought, and we must therefore question our own judgments of the universality of the notion. Granet notwithstanding, Mauss had conceived the notion of the person as a universal category of the human spirit. The question remains whether the person is a development of Western cultural history or an abstract category; Mauss had not adequately answered it, for the dualism in his method had not resolved the issue. We recall that at the end of his life, Lévy–Bruhl, whose dualism was of the content of human thought, purged himself of his dilemma by a direct negation of his own past.

The question had already arisen in Durkheim, according to whom the person is that which is impersonal in us. His formulation of the idea and its separation as an antimony and as a subjective transcendentalism is Kantian in origin and development. The controversy over monism and dualism in the totality of the work of Durkheim is posited in all its sharpness in this connection; his theory of the person is dualistic, although he also held that in the social milieu, the person as an individual may achieve a relative autonomy, not, however, as an intrinsic property but as an attribution from without, by the milieu.

In his essay on the person, Mauss referred to the overall program of the school of Durkheim, which was to explicate one by one the ten Aristotelian categories as those of the human spirit, an enterprise undertaken by Durkheim, in addition to Mauss, Lévy–Bruhl, Hubert and Czarnowski. They did not take into account the rigorous criticism of the Aristotelian categories advanced by Kant, who considered these to be both arbitrary and unclear in purpose. If, therefore, Durkheim was

too Kantian in his theory of the person and the human being, he was not Kantian enough in his program and that of his school. He was caught in the dilemma of monism-dualism, and, whereas his methodology was undoubtedly monistic, it was monistic by fiat and by example. His ontology is unclear on this point, despite defenses by Durkheim himself, by M. R. Cohen, C. Bouglé, Mauss and others.

The dilemma of monism versus dualism in the Western sciences of man was early posited not only in the Cartesian antinomy of mind and matter but also in the opposition of subjectivity and objectivity. In our tradition we have proposed that germane to man is the composition and opposition of subject and object; germane to our field is the opposition of subjectivity and objectivity. The universality of this opposition has been accepted as axiomatic in the dialectic, although Hegel himself posited, where possible, no axioms; to do so was to him the formalization of thought and therefore its death. Indeed, the opposition is questionable, and, although it may be found to be universal by empirical means, it will certainly be discovered in other strongly nuanced varieties of human thought, among them the opposition of agent to patient. In our discipline, however, the pairing of subjectivity and objectivity is a separation into parts that are rarely combined. The result has been a division within anthropology that is one in its subject matter but many in its doctrines.

During the past two decades the rhetoric of the social sciences has changed. The close relation of anthropology to psychology, psychiatry and psychoanalysis, which reached its peak in the 1930s and 1940s, was reflected in the projection of society onto the human psyche. Ruth Benedict's *Patterns of culture* was a product of that trend which, as developed in its broadest aspect, refined the study of the ideal, modal and type personality by mutually relating the study of society and the individual. Although that area of interest continues in force today, we are, with the growth of the field, proportionally less concerned with the explanation of culture in terms of the relation between culture and such spectra of personality and have instead come to speak of system, structure, model and code. But the latter language preceded the language and study of culture in its relation to personality, and we can explore its historical roots in Comte and Durkheim.

Max Weber had written on the subjective and objective methods in the subjective *(verstehende)* methodology of the social sciences but not in such a way as to bring the two methods together; Spencer had written

earlier on the objective and the subjective in the study of society but only in a negative way, mentioning the objective and subjective sources of difficulties in creating a social science. Comte and Durkheim, on the other hand, propounded the idea and methods of a purely objective social science.

The objectivity of Western social science is in part a development of its emulation, through Durkheim and others, of the natural sciences and is in counterposition to the *verstehen* of Weber. It is also a development of the character of Western social scientists who have thereby advanced their objective relation to the subject matter. The four-fold Stoic qualities of *ataraxía* (imperturbability), *adiaphoría* (tolerance) *apatheía* (dispassion) and *epokhé* (suspension of judgment) are the historical roots of this objectivity which has become the characteristic of our social science as a profession. These qualities are also found in different expressions and combinations in other civilizations with histories of the formation of professions, as in China, and these qualities are the professional qualities as such in the Oriental cases. They have become a means of training in the West and the East, and an end to be sought in professional training which has as its goal the quasi-objective value of judicial and more generally judicious neutrality, impartiality, tolerance or indifference and, in the literal sense, balance or suspension of judgment. These qualities are attributed by Hegel to the executive branch of the State, the bureaucrat being examplary of the Stoic virtues of equanimity, integrity and moderation.

The mandarinate of traditional China, the Stoic philosopher-emperor and the Hegelian bureaucrat had these qualities in common. The Stoic training and the preparation for the Chinese examination system also had in common the notion that the desired qualities and virtues were the features of the given cultural tradition, training in these qualities was conducted in the interest of serving the ends of the given cultural tradition and the human character as molded in the process had the intention not to divorce itself from its tradition but to strengthen its bonds with that tradition and perpetuate it. Training in the West for professional functioning in anthropology, sociology and so forth has the opposite intent, namely, to use the Stoic qualities of Western cultural tradition in order to achieve a divorce from that society and from any society. Thus, the professional body so constituted cannot allow sympathy with the experiences of the society observed to influence its professional judgment any more than can the body of physicians or lawyers do

so, for such sympathy is regarded as an intrusion into its professional sphere and action.

Here we have reference to the public and extrospective side of the Stoic and the mandarin traditions. In the private and introspective aspect the same qualities have at the same time led away from every professionalism, to flight from the public world, in which dispassionate *apatheia* is the opposite of all emotion, passion, affect, not only of the public kind but of all kinds. The public quest for the Golden Mean between the extremes of judgment and feeling was, in the inward life, the *lathe biosas*, the state of painless rest, eventually a mystical state. There is a warning of danger here to our anthropology as well, for once man is separated into the inner and the outer aspects as subject and object, he becomes an object and is set apart objectively. One part of our study will then develop increasingly in its objective form, and the other will develop into an inward-looking, possibly mystic, one. An echo of this opposition is to be found in C. P. Snow's two-cultures idea.

The aim of anthropology has been in major part to comport itself as do other sciences, objectively in regard to its subject matter. The result has been a partly illusory, partly real, success but without a careful exploration of the nature of either. The few general premises and conclusions of the social sciences are, however, common knowledge, commonplaces open to professionals and non-professionals alike. Yet the professional quest of professionalism is the same as it has been for ages, the *arcanum* which constitutes the group and excludes the laity. The same *arcanum* was the quest of the Pythagoreans who constituted in this sense – and not only in this sense – the opposition to Socrates. The dialectic of our discussion, only the first moment of which is posited here, is the interpenetration rather than the separation of the Pythagorean and Socratic approaches to the nature of man. The Socratic approach is the opposite of the arcane: The truth lies within the individual and will be revealed by the dialectic. To be sure, this is not our dialectic but the dialectic as maieutic, an external one. We counterpose the Pythagorean-arcane and the opposition to it of the Socratic-revealing. On the other hand, the service of the Pythagoreans to us is that they early established, to be sure not as the first, a standardization of discourse which they called their symbols and ensured the transmission of these symbols in unbroken line over three or four centuries in a number of places. To the outsider, the symbols were a meaningless babble and a rebuff, but to the Pythagoreans they were a means not only of determin-

ing the group, but also of performing its functions which advanced the control over nature.

The science of anthropology exists, as we have developed it in our discipline, not only as emulation but also in the separation from and interaction with the natural sciences. Heinrich Rickert, on the contrary, expressed in his theory of the limits of the formation of concepts of the historical and cultural sciences only the separation of the two and brought Edward Sapir into his camp. The historico-cultural sciences are, according to this view, sciences of the particular and therefore do not formulate general laws. In this connection, F. L. Hayek together with Ludwig von Mises have proposed a purely subjective science of economics and, by implication, of the social sciences generally, which in their view must of necessity rest on the knowledge of the subjective intent of a human action. Hayek sought to defend Rickert, who had denied the validity in the cultural sciences of the method of natural law, or causality, which he propounded as the method of *erklären*; more exactly, he referred in the investigation of history to its formal-logical aspect in which there could be no causal laws. Hayek noted that there are indeed general laws in the human sciences, although they are so simple and common place that they hardly seem worthy of being called theories. (In passing, permit me to say that this is scarcely a defense of Rickert or of Wilhelm Windelband, Wilhelm Dilthey, Georg Simmel and Max Scheler, who in various ways were all joined in the same separation, following the tradition of a Neo-Kantian dualism.) Hayek's proposal to defend Rickert is the story of the 'slightly pregnant' maiden. What Hayek in his so-called defense proposes is the end and destruction not only of Rickert but also of the sciences of man as a series of generalities which anyone can arrive at. To us this is but the beginning of our science which, by its professional activity, charges itself with the systematic and cumulative critique of these commonplaces.

There are many sides to this critique, which all of us are engaged in, and to two of which I have already alluded – the limitation of our theory as a cultural phenomenon and the limitation of our methods as subjective and objective. At this point I shall mention one more. Windelband had shown that the concept of the nomothetic is not that of a natural law but of a conventional construction derived from human effort. According to the Epicureans, *nomos* was simply a convention, while in the Stoic doctrine, *nomos* was a human law of normative validity in harmony with *physis* and nature and appearing as the positive con-

tent of morality. The Sophists went a step further, equating *nomos* with the human statute, and with *thesis*, the human institution. Nowhere did Windelband break with these traditional meanings in his juxtaposition of the idiographic and the nomothetic; to him, the nomothetic was a human convention, not a law of nature independent of its human construction.

Windelband's distinction between the idiographic and the nomothetic has had a strong influence on Eduard Meyer, Kroeber, Lowie and, with the qualification that he proceeded from description of the singular to the general, Max Weber. The idiographic explicitly bears upon the various, unique and independent lines of man-made history and culture; the nomothetic, as implicitly developed by Windelband, bears on man-made laws of external nature. An absolute – or if relative, at least a profoundly distanced – discontinuity is thereby interposed between nature and man, the objective as separated from the subjective. A dualism in method if not in ontology is therein proposed, as it is in Durkheim. Russell has made a distinction between objective law of nature and subjective principle of the mind, and his concept of a principle corresponds to Windelband's concept of *nomos* as law or convention.

Opposed to that dualist doctrine is the monist, in which the continuity of not only the subject and the object is posited but above all the continuity between the subjective and the objective processes. This continuity is engendered in the mutual transformation of the relations of subject and object; in addition both actually and potentially, the two are united in varying degrees in the observer and the observed. From this it follows that there is a continuity between all fields of human action, existence and development. Moreover, in the axiologies as theoretical and practical value constructions such as those of Linton and Kluckhohn, this continuity is extended to comprise the manifest and latent, the explicit and implicit, the overt and covert, as subjective and objective. The humanly real includes all participants in the relationships, for the real is not only the observable but also our own mental constructions. This conception of the anthropologically real and existent, therefore, is antidualist, for it comprises not only both actual and possible data of the sensorium but the humanly constructed as well.

The laws as conventions are at once the raw material of our discipline and the end result, the matter which we shape into our laws. The dual role of the *nomoi* is not a separation but a continuation, for we start from the monistic premise that all human thought is in a continuum

with itself and with its action, and our generalizations are of like matter with these as legislations and explicit norms and customs. But our generalizations are at once conscious and critical of themselves and are to be destroyed for a better, which is to go beyond the limit of Hayek's proposal.

Moreover, the separation of *erklären* from the methodology of the human sciences is an arbitrary narrowing of the term which not only alludes to a doctrine of causality but also denotes hermeneutic in general. Boas in his theory of myth and the folktale expounds them as both a recounting of and an accounting for a series of events, hence as both history and hermeneutic. Thus the combination of the opposition existed from the early time of the formation of anthropology as a profession.

Their composition is sometimes proposed to us from outside our discipline. The opposition of evolution and history, which is strongly felt by some, is not an opposition in the biological work of A. S. Romer and others. In their conception, the two terms refer indifferently to the factor of time in the scientific investigation of living matter, posing therewith an unresolved problem in anthropology, for in our discipline evolution and history stand opposed to each other. Romer's view is that time in biology is absolute or, if relative, relative only to physical time; as such, it is an *a priori* category like space. The mathematical model of time as linear continuity applied in biology is held by most mathematicians and mathematical physicists, including relativists. Temporal process, in the conception of some anthropological evolutionists, is relative to the development of a surviving trait or of a particular culture in general, for it has been separated from process in historical time and conceived as subject to its own inner dynamism, hence as subjective. On the contrary, temporal process of the biologists mentioned, having its course in time as defined above, is objective. And if it is conceived as history, it may be generalized and is therefore opposed to the particularizing theories of historical process of Dilthey and Rickert. The problem of evolution in anthropology, including the single evolution of the species and genus and the evolution of culture, whether singular or plural, remains an unresolved problem of our discipline, not so much because of paucity of data as because it has been incompletely analyzed.

In our anthropology, both evolution, conceived as an unfolding determined by general law, and history, conceived as a plurality of recountings

or interpretations, have traditionally been directly concerned with the notion of coming into being, which we have had transmitted to us from Aristotle and from Hegel. Structualism, on the other hand, whether synchronic or diachronic, whether conceiving the structures as socially real or as the mental constructs of a *tertium gaudens*, has been without direct involvement with the problem of development as temporal process. As such it has had little to do with the matter of time in culture or with the problem of cultural florescence, that is, with the realization of cultural potentials and their exhaustion, hence little to do with coming into being and passing away. Any scientific discipline strives as much for unity as for diversity within itself; the interrelation of evolution and history with structuralism is proposed as a synthesis of current theory by reconceiving social structures in terms of process or processes of development. The potency of anthropology in any cultural tradition is criticism, which may be set forth as a threefold matter. First, the limitations of the cultural tradition which have generated our discipline are made conscious and explicit by its practitioners, or may be. We have the potentiality to break away from mental custom, to be stripped bare, as far as possible, of the presuppositions of our civilization and its judgments of particular value. In realizing this potentiality of our tradition, we show the skewing of the human handiwork, of what man has made of man, and what has been omitted and committed by the realizations that we have developed within our cultural frame. Next, the power of critique may be brought to bear as much upon the custom, thought and institutions of the ethnographized society as upon those of the ethnographer, parallel and opposite to the foregoing. Finally, anthropology has been accused of being a conservative doctrine for its devotion to the preservation of the record and its reasons as a subtle service in the preservation of what is, a service which we have had borne upon us from Leibniz and in part Hegel. But the record that we set down includes the negative voices and the acts of rejection. Most of our subjects are poor, oppressed and illiterate; some of them are disenchanted and disaffected and increasing numbers are revolutionary. The recording and interpretation of these voices and acts are as much our task as is the distanced and objective anthropological activity which has been caricatured as scientism. The promulgation of critique on these levels and on othere, for the critique is not at an end, will determine for us that we have exceeded the given state of man as we find him and will spur our theory beyond the mere reflection of the given state.

REFERENCES

Arensberg, Conrad (1965), Chap. 1. In. C. Arensberg and Solon Kimball, *Community and culture.*

Boas, Franz (1938), *The mind of primitive man.*

— (1914), Mythology and folk-tales of the North American Indians, *Journal of American Folklore* 27. Reprinted in Boas (1940), *Race, language and culture.*

Bogoraz, Vladimir (1904–1099), *The Chukchee: American Museum of Natural History,* Vol. 11, *Memoirs.*

Brunschvicg, Léon (1927), *Le progrès de la conscience,* 2 vols.

Comte, Auguste (1838–1842), *Cours de philosophie positive,* 6. vols.

Durkheim, Emile (1912), *Les formes élémentaires de la vie religieuse.*

— (1898), Représentations individuelles et collectives, *Revue de métaphysique et de morale,* 6.

Feng Yu-lan, (1952–1953), *History of Chinese philosophy,* 2 vols. D. Bodde, Jr. (Ed.)

Fortes, M. (1953), Structure of unilineal descent groups, *American anthropologist* 55.

Gierke, Otto (1868–1913), *Das deutsche Genossenschaftsrecht,* 4 vols.

Granet, Marcel (1934), *La pensée chinoise.*

Hayek, F. A. (1962), Introduction to H. Rickert, *Science and history.*

Hegel, G. W. F. (1821), *Grundlinien der Philosophie des Rechts.*

Kluckhohn, Clyde, Murray, H. A., and Schneider, David (1953), *Personality in nature, society and culture.*

Krader, Lawrence (1966), Primary reification in primitive mythology, *Diogenes,* Oct.

— (1967), Persona et culture, *Les Etudes philosophiques,* November.

Kroeber, A. L. (1917), The superorganic, *American anthropologist,* 19.

— (1952), *The nature of culture.*

— and Kluckhohn, Clyde (1952), *Culture: A critical review of concepts and definitions.*

Lévi-Strauss, Claude (1962), *La pensée sauvage.*

— (1965), *Mythologiques: Le cru et le cuit.*

Linton, Ralph (1936), *The study of man.*

Mauss, Marcel (1950), Une catégorie de l'esprit humain: La notion du personne, celle de moi, *Sociologie et Anthropologie.*

Mumford, Lewis (1961), *The city in history.*

Needham, Joseph (1954–1956), *Science and civilization in China,* 2 vols.

— (1941), A biologist's view of Whitehead's philosophy. In P. Schilpp (ed.), *The philosophy of A. N. Whitehead.*

Odhiambo, T. R. (1967), East Africa: Science for development, *Science* 58, 1967.

Pohlenz, Max (1948–1949), *Die Stoa,* 2 vols.
Rickert, Heinrich (1929), *Die Grenzen der Naturwissenschaftlichen Begriffsbildung,* 5th ed.
— (1962), *Science and history.*
Ricoeur, Paul (1963), *Archivio di Filosofia.*
Sapir, Edward (1917), Do we need a superorganic?, *American Anthropologist,* 19.
Sartre, J. P. (1960), *Critique de la raison dialectique.*
Weber, Max (1947), The fundamental concepts of sociology. T. Parsons, (Ed.), *The theory of social and economic organization.*
Whitehead, A. N. (1929), *Process and reality.*
— (1925), *Science and the modern world.*
Wundt, Wilhelm (1900–1909), *Völkerpsychologie,* 5 vols.
Zeller, Eduard (1923), *Die Philosophie der Griechen,* 3 (part 1).

2 Anthropological traditions: Their uses and misuses

JEAN POUILLON•

To inquire into 'the nature and function of anthropological traditions, is to admit first, that such traditions exist; second, that anthropological research is to some extent determined by these traditions; and third' that anthropologists are not biased observers whose descriptions and analyses are oriented, if not falsified, by their cultural affiliations. Finally, such inquiry is to hint, it seems to me, at an optimistic answer, namely, the anthropologist does not, as he would like to believe, escape his ethnocentrism, but he can make good use of it. 'If society is in anthropology, anthropology itself is in society' (Lévi-Strauss, 1960, p. 46). But is that good or bad? In either case, it is a *de facto* situation that we must be aware of if we wish to determine the truth value of anthropological knowledge. The first part of the sentence quoted formulates the theoretical aim of our investigation; the second indicates the necessary point of departure. The entire problem is to determine whether in acknowledging the anthropological enterprise as problematical we condemn it to failure or whether we can establish, or at least leave open, the possibility of its success.

In other words, we are invited to question an admittedly naive belief in the objectivity of the anthropologist – a belief that is doubly naive in that it ignores the concrete circumstances of the investigator and confuses the objectivity of knowing with the neutrality of recording in order to have a better understanding of the nature of this objectivity. The anthropologist is a member of a society of which he always bears the unconscious mark – a certain conception of the world or, in less indulgent terms, a collection of prejudices. Of course, experience in the field aims, if not always successfully, to strip the anthropologist

of his ideological dross. But it would be vain to hope that he might be reduced to a mere onlooker. Doubtless there are prejudices that blind, but we are no more clearsighted if the eye we turn upon reality is not 'directed' or prepared. In fact, it always is. The particular situation of the observer, whether he wishes it or not, whether he realizes it or not, controls what he sees and selects from his environment what he will retain. Every observation is choice. It is illusory, therefore, to claim to refuse to choose, but it is indispensable to ask what our criteria are. Apparently, we can either look for them at home or borrow them from the outside, from the people we study. It would be pleasant to think that the alternative is practicable in both its bifurcations, that it is possible to see things from the very point of view of those we seek to understand and that we can, in some way, become one of them, change our skin. How many fine phrases have been written on the necessary humility of the ethnographer and the mimetism which supposedly guarantees the validity of his descriptions! However, it is not only doubtful that such a change of roles is really and fully possible but, above all, that it is desirable. No society has the privilege of lucidity, and 'natives' equally have their ideologies, the reconstitution of which by the ethnographer, if he means to limit himself to that, would merely be 'the lazy refuge of a vacillating thought. ... Ethnography would then dissolve into a wordy phenomenology, a falsely naive admixture in which the apparent obscurities of native thought would be advanced only to cover the otherwise too glaring confusions of the ethnographer's own' (Lévi-Strauss, 1950, p. XLVI). Besides, why should we exchange one system of prejudices for another? It is a question, rather, of learning to utilize each in order to understand the other and, perhaps, to detach ourselves from both. How? In another form, we always come back to the same question, and it is probably this salutary procedure that Rabelais and Montaigne had in mind when, in order to contest those of their own time and country, they exuberantly enumerated the strangest and most alien customs they knew of. In so doing, they sought less to know others than to know themselves; the *other* enabled them to know *themselves in another way.*

Doubtless, this is not the main aim of anthropology, and it would be rather forced to make of Rabelais and Montaigne anthropologists *avant la lettre.* But this knowledge of the self as singularity cannot be dissociated from knowledge of the other, and it is only when one has seen oneself as other that it becomes possible, without ethnocentrism,

to see the other as the same as oneself. That is why their lesson is fundamental and of direct concern to our subject: We merely make an empty vow if we say that we must begin by ridding ourselves of common habits of thought in order to be able to approach an alien society, for it is impossible to satisfy such a preliminary condition; it can be effected only *in*, and not *before*, the ethnographic work. It is useless either to want or to claim to take off our spectacles before going into the field, for we do not even feel we are wearing them, particularly if we imagine we have already taken them off. It is only in the field that we discover we are wearing them, and then it is not a matter of taking them off but of using them deliberately, of establishing a right relation between the observer and the observed, one that might enable understanding while maintaining difference and that might disperse the symmetrical and inverse illusions of projection and identification. It is a question, indeed, of a relation, of a distance which anthropological knowledge must traverse in two directions, not claim to abolish.

'The ethnological problem is ... a problem of communication' (Lévi-Strauss, 1950, p. XXXII) between two terms that remain different even when they meet. 'When we live with autochthons and speak their language, by learning to represent their experience in their way, we approximate their thought as much as we can without ceasing to be ourselves. On occasion, we try to represent their conceptions in a systematic way with the help of the logical constructions that our education has taught us to use; and in this way we hope, at best, to reconcile what can be expressed in their languages and what can be expressed in our own. We mediate their habits of thought, which we have acquired with them, and those of our own society; by proceeding thus, it is finally not some mysterious primitive philosophy we are studying, but the extreme potentialities of our own thought and our own language' (Lienhardt, 1954, pp. 96–97).

The most neutral ethnographer cannot limit himself, no matter what he says, to describing or, rather, he cannot describe at all. The most classical monographs are already comparative works in which the authors' own categories play as much a part as those of their informants. To be convinced of this we have only to look at their tables of contents – the order, to which the material to be communicated does not always easily lend itself, responds to the mental exigencies of the readers. This statement of fact is by no means a reproach: The author writes down in his own language what has been told to him in another, and,

as we know, every language has consequences that are not simply linguistic. Translation must be faithful and intelligible – faithful to the 'language of departure', intelligible in the 'language of arrival'. This conjunction is not so easy to effect. Ordinarily, we insist upon fidelity, and so we should, but it is also important – and this is frequently forgotten–to become aware of the transformations and transpositions that intelligibility supposes. We neglect them because we too easily admit the neutrality claimed by so-called scientific language. Now, this neutrality is not given; it can be reached only after one has reflected on oneself and on the traditions imperceptibly conveyed in the language we employ.

The notion we have of others is a function of what we are ourselves; this, as we have just seen, is the first difficulty encountered by the anthropologist. He can overcome it – that is to say, transform this 'prejudiced' idea into true knowledge – if he knows himself and integrates these two knowledges, of the self and of the other, into what then becomes, properly speaking, an 'anthropology'. This means admitting that he can become conscious of the traditions which orient his thought, that he can judge them and need no longer submit to them even if he still accepts them.

But can we, indeed, admit of such a possibility? Everything depends on what is meant by tradition. Tradition normally suggests a continuation of the past in the present, a preformation of the latter by the former, a preformation the effectiveness of which pertains to its unconscious character. At the same time, we affirm that this unconsciousness can be dissolved. It is at this point that a new difficulty appears: How do we know that it can? How can we be sure that we have completely revealed the determinations of our attitudes, of our methods, of our theories? Is it enough to have *once* formulated the hypothesis of an unconscious causality for it to be impossible *ever* to be rid of it, for it to be impossible to affirm the independence of our thought at all? Doubtless, matters are more complex: This causality is not mechanical, traditions do not explain everything and to belong to a tradition is not simply to repeat it; it can also be to transform it. But the objection stands: To believe that we have managed to assume a distance from our past may be a trick of the unconscious.

We can leave this impasse only by putting into question the definition of tradition as the past acting on the present and the definition of its study as the quest for a causality that can be expressed. chronologically. Actually, what is unconscious in a tradition is that it is precisely

the work of the present looking to the past for a guarantee, a guarantee which seems more 'bourgeois' for being conceived on a model of causality and not on one of reconstruction. Ethnologists, moreover, should be the first to know this, for they study societies that they call 'traditional' when, paradoxically, they know nothing, or very little, of their history. 'We do this because our grandfathers did the same thing'; this typical answer, disappointing on first judgment, is intellectually the most satisfying possible, especially when, as often happens, we have the good fortune to discover that it is historically inaccurate, that their grandfathers behaved differently. It is then that one understands that such an answer is the expression not of the stupid observance of stereotyped rules but of the consciousness of the group of its structured character: It signifies that the behavior in question relates to all other forms of behavior and that the global system has no other justification than its own existence which is then projected into the past and cannot be invalidated by any inaccuracy of detail. In our literate and therefore conservative societies, the weight of the past is much heavier; that is why, doubtless, in compensation, we value change so highly. But finally, since we keep everything, we must be selective. Tradition for us, too, is a 'retro-projection': We choose what we say we are determined by; we present ourselves as the heirs of those we have made our predecessors.

Consequently, to define a tradition we must go from the present to the past and not vice versa; we must understand it not as a *vis a tergo* to whose effects we submit but as a point of view from which we look at the past. By this, I do not wish to imply that recognizing a tradition is the same as inventing it. The past must persist for us if we are to make use of it, and we cannot draw from it an infinity of possibilities. The past imposes the limits within which our present interpretations of it depend. And these interpretations are not necessarily anachronistic; it is not a matter of stamping the present upon the past but of finding in the past the germ of solutions that we consider correct today, not because they were thought to be correct then but because we believe them to be correct now. To become conscious of a tradition means finding a heritage in the past by means of our own criteria. It is normal that it should be so, but normal also that we should not always be aware of it: Culture opposes nature, but willingly thinks of itself as 'natural'; by the same token, the march of tradition goes against the biological hereditary principle but is often modeled on it. In fact, tradi-

tion is an inverted filiation: The son in this case begets the father, and that is why he can endow himself with several fathers!

The anthropologist finds himself in the same situation with respect to culturally different contemporaries as to culturally similar predecessors: He no more passively records the modes of life of the first than he repeats the modes of thought of the second. It is through the same centrifugal movement, but based on two different axes – one spatial, the other temporal – that he gives meaning to the works of both of them. Obviously, in neither case does this guarantee his objectivity, but it eliminates a false problem, that of the distorting influence of traditions on the work of the anthropologist. And above all, 'the nature and function of anthropological traditions' now emerge more clearly. Through his interpretation of a given author, the anthropologist expresses what he has done or wanted to do in the field; he formulates in terms of his own culture the lesson he has derived from contact with alien cultures. By choosing precursors, he reveals the way in which he approaches cultures, and, vice versa; his work in the field reveals the way in which he reads authors. In brief, anthropological tradition is not a framework within which every anthropologist is constrained to set himself; rather, it offers the anthropologist an opportunity for self-justification.

The proof is that there is not *one* tradition but *several.* Their multiplicity shows that their role is not the accumulation of experiences and doctrines into a kind of global mass which would constitute *the* tradition and which would be the common property of everybody. On the contrary, traditions are discriminatory, and that is their essential and practical function: By affirming that he is affiliated with such and such a tradition, an anthropologist does not simply exercise a retrospective choice; he *first of all* makes a present choice; he puts himself in opposition to certain colleagues as well as expressing agreement with others. To have recourse to a tradition is a way of formulating one's difference. At bottom, different traditions should be systematized; we would doubtless end up with a totemic system in the meaning that C. Lévi-Strauss (1962b, p. 302) gives to the word – a system of references which are more important for the distinctions they delineate than for the resemblances they establish. That is why it matters little that claiming a relation to an ancestor might sometimes appear somewhat forced, since its importance depends upon its diacritical value. In short, anthropological traditions are classifications.

It follows that a study of these traditions cannot be historical and

cumulative. I shall not, therefore, undertake even a summary history of anthropology in France, and I shall not enumerate all those who, on the basis of different claims and to different degrees, can be qualified as anthropologists or precursors of anthropology. I shall try rather to show how a certain way of conceiving anthropology today – let us say structuralism – allows a certain reading of our past. I shall not retrace the complex genealogy of structuralism. However, were I to do so, such an investigation would go beyond the limits of this paper. Moreover, it would probably become evident that structuralism is in some way constituted dialectically, in opposition to other tendencies and not, so to speak, as a natural development of pre-existing ideas. In truth, this is always the case: A new theory is constituted not from an anterior and analogous theory that contains its seed but as the answer to an unresolved problem or, more often, to one not yet stated. That is precisely why its history has nothing to do with the emergence or the development of a tradition. On the other hand, every important new theory, however it was constituted, throws new light on the past and, so to speak, creates its own tradition.[1] I shall begin with a quotation, which seems simple and even banal, scarcely characteristic of a particular attitude, but which permits in reality the coordination of a certain number of themes that illuminate our readings of past authors. In his inaugural lecture at the Collège de France, C. Lévi-Strauss (1962b, p. 46) writes that the role of anthropology is to put man as a whole into question, in each of his particular 'examples'. That means, first of all, from an ethical as well as from a scientific point of view that none of the 'examples' of man may be privileged nor should they be classified in reference to the 'European' or 'Western' tradition. The European represents neither the norm of humanity – in relation to which other men represent extremes or aberrations – nor its highest manifestation – the last stage of a development that other men are incapable of or that they have not yet reached. Savagery, barbarism, civilization are relative and subjective notions, the futility of which was already quite apparent to Montaigne (1946, p. 213): 'There is nothing barbarous and savage in that nation [the Indians of Brazil], from what I have been told, except that each man calls barbarism whatever is not his own practice.'

1. A good example is N. Chomsky's view in *Cartesian linguistics*. Chomsky interprets the past in light of the theory he has elaborated while criticizing his colleagues. It is because of his being Chomsky that he reads Descartes as he does, but it is not through his reading of Descartes that he has become Chomsky.

As I have already remarked, it is less a question for Montaigne of knowing alien practices in themselves–even if his information was accurate[2]–than of contrasting them with our own in order to show that ours are neither more natural nor more moral: 'I think there is more barbarity in eating a man alive than in eating him dead; and in teasing by tortures and racking a body still full of feeling, in roasting a man bit by bit, in having him bitten and mangled by dogs and swine (as we have not only read but seen within fresh memory, not among ancient enemies, but among neighbors and fellow citizens, and what is worse, on the pretext of piety and religion), than in roasting and eating him after he is dead' (Montaigne, 1946, p. 217). As we read this passage from the *Essais*, another text written several centuries later springs to mind: '... certain of our own usages, if investigated by an observer from a different society, would seem to him similar in kind to the cannibalism which we consider "uncivilized". I am thinking of our judicial and penitentiary customs. If we were to look at them from the outside, it would be tempting to distinguish two opposing types of society: those which practice cannibalism – who believe, that is to say, that the only way to neutralize people who are the repositories of certain redoubtable powers, and even to turn them to one's own advantage is to absorb them into one's own body. Second would come those which, like our own, adopt what might be called anthropoemia (from the Greek *emein*, to vomit). Faced with the same problem, they have chosen the opposite solution. They expel these formidable beings from the body public, by isolating them for a time, or forever, denying them all contact with humanity, in establishments devised for that express purpose. In most of the societies which we would call primitive, this custom would inspire the profoundest horror; we should seem as barbarian to them as they to us' (Lévi-Strauss, 1955, p. 418) on the ground of no more than symmetrical customs.

One of the persisting characteristics of anthropology in France is this self-questioning to which ethnography has contributed from its earliest beginnings, even before it was scientifically constituted. It is enough that there are others, even if we remain ignorant about them, for us to be able to criticize ourselves and to divest our customs of their false testimony. Montesquieu has no need to know Persia to ask what

2. On this matter see Métraux (1963, pp. 721–738). In this article, A. Métraux recalls the works of the first French travellers and missionaries who described the manners and customs of the South American Indians.

it is like to be Persian, since his true aim is to show how strange it is to be Parisian. Similarly, Diderot was not really concerned about the Polynesians. But the very progress of our knowledge has only reinforced and justified this tendency, which is both at the beginning and at the end of ethnological research: The anthropologist is a man who achieves distance from both himself and his society of origin. Rousseau (no date, Ch. 8), who, interpreted today by C. Lévi-Strauss, takes on even greater stature, has said it better than anybody: 'When one wants to study men, one must look close to home; but in order to study man, one must learn to look far afield; one must first of all observe differences in order to discover common properties.'

Such a critical perspective is not unrelated to a synchronic view of the human universe, to the absence, in any case, of all evolutionist perspective, for two reasons, one moral and the other logical. The moral reason is that linear evolutionism is evidently incompatible with self-questioning. The evolutionist feels at ease in his society; he approves the course of its history since, whatever the imperfections he is quite willing, at least provisionally, to recognize in it, he nevertheless sets it at the top of the ladder of existing societies and sees in it the path that all human groups must follow. In evolutionism there is a self-satisfaction completely alien to men like Montaigne, Montesquieu and, above all, Rousseau or, to take a modern example, Michel Leiris (1934).

The logical reason is no less determining. If it is 'man as a whole' we must implicate in each of his manifestations, it is because in each man we find the 'whole' man because each society represents a singular but perfected realization, containing its own meaning, of man. On the other hand, evolutionism obliges us to consider each realization of man as a phase, whose meaning is outside it, within evolution as a whole; in each phase, man is defined by his inadequacies, by his incompleteness. Then, again, evolutionism supposes a sense of temporal depth which, after all, is quite recent, its source being in Darwinian transformism and the development of palaeontology with Boucher de Perthes, that is to say, originating in the middle of the nineteenth century. Before that, the 'savage' is considered a contemporary, not the witness, of an archaic stage; 'he may be our brother, he may be our cousin, but he cannot be our great-grandfather.'[3] Doubtless it often seems that

3. See Tinland (1968, p. 17). It is true, however, that this book is more concerned with conceptions of man as a species than as a social being.

he can be historically situated in relation to a common primitive state from which all men, including ourselves, have diverged–the state of nature. It is thus that Montaigne (1946, p. 214) writes: 'Those nations, therefore, seem to me barbaric in having been little fashioned by the human mind and in being still close to their original naivete.' In reality, this reference to a hypothetical state of nature is only apparently diachronic. Its true function is, if not to resolve, at least to formulate the problem of unity in spite of difference. To return to our formula: If man is in each of his examples, it is because each example in its very particularity has in common with all the others that it is an example of man. In other words, it means imagining cultural relativism which, at its limit, would be inconceivable.

The reference to nature as it is found in Montaigne and many other writers does no more than state, without definition, a basic community. But how can we conceive the singular as such, and how can we explain the diversification of societies? The problem was neither easy to formulate nor easy to resolve for the eighteenth century because the philosophy of the Enlightenment, according to which the progress of civilization was conceived on a single model, was to dispel the absurd beliefs and superstitions that everywhere beclouded the mind. In keeping with his premises, Voltaire had only disdain for savages. Diderot, on the other hand, idealized natural man, the 'noble savage', the better to contrast him with social and artificial man; thus, Diderot condemned himself to understanding nothing of the material brought back by the first ethnographers. The attitude of Montesquieu is incontestably more scientific, and he has too often been seen as a father of social anthropology for me to say more than a few words about him in order to show what, in *L'Esprit des lois*, might today be of interest to structuralism. Let me note first of all that his aim is not polemical – 'I am not writing in order to censure what is established no matter where' (1951, p. 230) – and that he proposes only to understand the reasons behind the maxims that are found in every nation. These maxims are always particular; they define each people in its singularity. Nevertheless, nothing would be more mistaken than to make of Montesquieu an ancestor of what has been called 'culturalism' which characterizes each society by a characteristic or complex of characteristics that are not found elsewhere. Each social configuration is original, less, however, for the materials of which it avails itself than for the way it apportions and organizes them. Montesquieu (1951, pp. 227–228) makes this clear from the outset

in his prefatory note: 'It should be noticed that there is a vast difference between saying that a certain quality, modification of the mind, or virtue, is not the spring by which government is actuated, and affirming that it is not to be found in that government. Were I to say such a wheel, such a pinion is not the spring which sets the watch going, could it then be inferred that it is not to be found in the watch? ... In a word, honor is found in a republic, although its spring is political virtue; and political virtue is found in a monarchy, although its spring is honor.'

The knowledge of particular laws is therefore a general knowledge capable of systematization. The spirit of the laws is the ordered ensemble of all the relations that civil and political laws maintain with the nature and principle of government of a specific people, on the one hand, and, on the other, with the physical features of a country, climate, size, way of life, religions, customs of the inhabitants and so on.

This definition makes it clear how absurd it would be to claim that one nation is closer than another to the state of nature or to a state of reason. All are equally natural, equally reasonable, and therefore equally intelligible. Yet it must be asked if these views, correct in theory, do not oversimplify the analysis: On the one hand, the abstract and too scanty assemblage of forms of government and, on the other, the pure diversity of *données* ordered quite arbitrarily by an often contestable ecological causality. In fact, the teeth of the typological comb are too widely spaced, while the comb itself is too narrow: It applies only to state-structured societies, and most of the best examples are borrowed from the peoples of Europe or from classical antiquity. This last criticism is unfair to the extent that it does not take into account the poverty and uncertainty of ethnographic information in Montesquieu's possession even though, as Démeunier was to show not long after, more could have been deduced from what was available.

Rousseau was perfectly aware of this situation, as we see from his note 10 to the *Discours sur l'origine et les fondements de l'inégalité* (1965, pp. 172, 174) in which C. Lévi-Strauss (1962a) correctly finds the first definition of ethnology:

> For the three or four hundred years since the inhabitants of Europe have inundated the other parts of the world, and continually published new collections of voyages and reports, I am convinced that we know no other men than the Europeans; furthermore, it appears, from the ridiculous prejudices which have not died out even among men of letters that under the pompous name of the study of man everyone does hardly anything except study the men of his country. ... One does not open a book of voyages without

> finding descriptions of characters and customs. But one is completely amazed to see that these people who have described so many things have said what everyone already knew, that they have known how to perceive, at the other end of the world, only what it was up to them to notice without leaving their street; and that those true features which distinguish nations and strike eyes made to see have almost always escaped theirs. Hence this fine adage of ethics, so often repeated by the philosophical rabble: that men are everywhere the same; that as they have the same passions and the same vices everywhere, it is rather useless to seek to characterize different peoples–which is about as well reasoned as if one were to say it is impossible to distinguish Peter from James, because they both have a nose, a mouth, and eyes. ... I have difficulty conceiving how, in a century taking pride in splendid knowledge, there are not to be found two men ... one of whom would sacrifie twenty thousand crowns of his wealth and the other ten years of his life to a celebrated voyage around the world in order to study, not always stones and plants, but for once men and morals... .

But Rousseau did not simply sketch the ethnological program, he established its very possibility. I can do no better here than to refer back to the text by Lévi-Strauss already mentioned as well as to Chapter 28 of *Tristes tropiques*, which illustrates in a particularly clear way what I have tried to show, namely, that a tradition is always the work of the one who claims it as his. When indeed Lévi-Strauss refers to Rousseau, it is in order to resolve problems that arise from his experience as an ethnologist, and it is this experience which enables him to read Rousseau well; what are the relations between myself and the other, between nature and culture, and what is the positive significance in the fact that every ethnologist, as we have seen, puts his own society on trial. Only an ethnologist could find in Rousseau any other than a psychological answer to the first question, any other than a historical and political answer to the third, and, above all, he alone, *à propos* of the second, could correct an error which, often repeated, would have passed for truth, were tradition established by transmission. Contrary to what is frequently claimed, Rousseau did not desire to reconstitute an historical genesis beginning with a state of nature in which man lived before the formation of the simplest social organization; he did not exalt natural man. His aim was to make the present social state intelligible and for that purpose to distinguish 'what is originary and what is artificial in the present nature of man'. According to this formula, nature is not anterior or exterior to society; natural man is immanent in social man, without whom Rousseau's investigation would

be impossible. In other words, the opposition between nature and culture is not historical but methodological; it is therefore just as much a relation which persists in every man, in every society. The problem is knowing what belongs to each of these two terms, although they cannot exist separately.

Rousseau's answer prefigures that of Lévi-Strauss in Chapter 1 of his *Structures élémentaires de la parenté:* A rule is social when it determines behavior that the biological nature of man does not control. Hence, the diversity and variability of social institutions – the object of ethnology – which offer themselves not only to scientific study but also to moral judgment: What should a viable society be? We can answer this question only if we can isolate the faculty in every man that articulates nature and culture. This faculty, according to Rousseau, is pity which, by urging the individual to go beyond himself, makes him accept himself in the other. It gives rise to a behavior that natural needs do not explain; it is at the basis of the exchange and reciprocity which originally make up the social state, inherent, apparently, to the nature of man. But nothing predetermines the concrete modalities of exchange and reciprocity since it is precisely these factors that differentiate social groups. It is thus to their discovery, beneath the disparities and contradictions occasioned by their formation under different material conditions and subsequent development, that the anthropologist must apply himself. But he can succeed only by repeating for himself, following the example of Rousseau, the constitutive movement of culture and society: 'to refuse oneself in oneself' in order 'to accept oneself in others' (Lévi-Strauss, 1962a, p. 242). Hence, the resolution of the apparent paradox – that cannot be sufficiently explained by the psychology of the ethnologist – that urges the ethnologist to criticize the society of which he is a member so as to free himself from it, whereas he accords patience and respect to different societies which he would reject just as strongly if he were a member of them.

After Rousseau, what was necessary for anthropology to be constituted in France? Doubtless a less kaleidoscopic perception of different customs, presented as so many isolated, unorganized curiosities, whose place in the social system from which they were extracted was neglected. Doubtless, also, these customs should have been studied for themselves and compared with one another without preconceived ideas, whereas they were compared with the societies of peoples of antiquity – Jews, Egyptians, Greeks, Romans – and were thus made to serve the

particular history of the ancient world. This concern is revealed in the titles of numerous works: *Du culte des dieux fétiches, ou Parallèle de l'ancienne religion d'Egypte avec la religion actuelle de Nigritie*, by Président de Brosses; *Moeurs des sauvages américains comparées aux moeurs des premiers temps*, by Père Lafitau; and *Conformité des cérémonies chinoises avec l'idolâtrie grecque et romaine*, by N. Alexander. However, in 1776 a book appears which breaks with the productions of the time: *L'esprit des usages et des coutumes des différénts peuples, ou Observations tirées des voyageurs et des historiens.* Its author, Jean-Nicolas Démeunier,[4] was twenty-five years old; he had never traveled but earned his living translating travel tales that had come out in England. No doubt he was sensitive to the haphazard character of material that had been accumulated without any thought for classification or systematic comparison. This is what he undertook in his book which was successful enough to go into further editions in 1785 and 1786 but which fell into oblivion and is today difficult to obtain. Following this work, Démeunier tirelessly continued translating, continuing the work of the Abbé Prévost, *L'Histoire générale des voyages* (twenty volumes published from 1746 to 1801), 'a cellection of all the accounts of voyages on land and sea that have been published up till now in different languages and in all known nations'. In 1785 Demeunier wrote the economic section of the *Encyclopédie méthodique*, edited by Panckoucke, and the following year published an essay on the United States. He was Deputy for the Third Estate of Paris at the Estates General in 1789 and a member of the Constitutive Assembly. He resigned in 1791, left for the United States and did not return to France until after the Terror. He figured on the list of candidates for the Directory. He died in 1814, a senator and count of the Empire.

Démeunier was not completely forgotten. Radcliffe-Brown (1958) recognized his originality and ranked him among the founders of social anthropology: '... [W]hile writers on the usages of other peoples have been concerned only with their bizarre and ridiculous aspects, he proposes a new method by which we seek their spirit. Démeunier was one of the founders of social anthropology... . For Démeunier the comparison

4. I am indebted to Mrs. Edna Lemay for generously allowing me to consult her notes on Démeunier and for providing me with a copy of Démeunier's book which is extremely difficult to obtain. Mrs. Lemay is at present working on a book on Démeunier.

of usages of different peoples would enable us to discover what he called their spirit or what some might now call their meaning.' Van Gennep (1910 and 1914) also gave him his due. When Démeunier has good material at his disposal, 'he gives acceptable solutions. His discussion on the extent and significance of cannibalism, denied by his contemporaries, is good. Similarly, having read in Kolben's report from the Cape that young Hottentots, when they become adults strike their mothers, he recommended that we should not be outraged but take it as a simple ceremony marking their emancipation.... His studies on the impurity of women, on the division of labor according to sex ... can be read with interest'. Van Gennep concludes that, thanks to Démeunier, general anthropology was constituted in France in the eighteenth century: 'One of the most durable virtues of the eighteenth century was to return man, who had become separated from other objects and beings by theology and mythology, to nature. Were it not for the religious recrudescence of the first three quarters of the nineteenth century, ethnography in France ... would occupy the position of honor which is its due and which we hope to see it acquire soon.'

Travellers did not present their information in ways which allowed Démeunier to compare social systems; his materials were, so to speak, atomized, and he could only regroup usages. But, as his table of contents indicates, his work implies a certain conception of the internal unity of every social group which might have provided ethnographers with a complete framework of inquiry. Indeed, he sought to classify usages not according to their similarities or differences but according to their social functions. By the same token, similarities and differences among nations, which in themselves mattered little to him, could be explained either as different customs which had similar functions or as analogous customs with different purposes. Man is primarily a being who subsists and perpetuates himself; therefore Démeunier first of all considers what and how he eats his food and his table manners; then the condition of women, forms of marriage, birth customs and education of children. Man is also a being who lives in a group; therefore it is appropriate to deal with the different parts of the social body–chiefs and sovereigns, warriors, masters and slaves, distinctions of rank and property. Man is a being with symbolically expressed relations which he regulates. Thus Démeunier mentions the different conceptions of beauty and adornment, of modesty; then he deals with domestic usages as well as penal laws and their administration. Ultimately, man is mortal,

and the work ends with considerations on homicide, suicide, human sacrifice, sickness, medicine, death and funerals.[5]

The book's title – *L'Esprit des usages* – obviously recalls Montesquieu, whom Démeunier quotes on several occasions with great respect. Nevertheless, as Van Gennep pointed out, he is far from applying, purely and simply, the method of *L'Esprit des lois*. He at once deviates from it by stating that one cannot automatically and in every case directly explain a custom in terms of a natural or intellectually intelligible cause. Every custom belongs to a system, and every system evolves, but it evolves as system, that is to say that soon its elements can only be justified synchronically, by the position they occupy in the whole, without it being possible to assign an external cause to them; hence, those peculiarities which intrigue superficial observers in quest of a genetic explanation. On the other hand, Démeunier realizes that cultures are just as much the means whereby societies affirm their singularities with respect to one another as they are responses to natural conditions. Thus the anthropologist fulfills his task not by conjecture about an uncertain history but by noting the differential variants which define cultures. Van Gennep nevertheless talks of the evolutionism of Demeunier. In fact, he affirms that every society evolves according to the same *formal rules* – of complication and of sophistication – but the diversity of customs nonetheless remains and demands a synchronic, or rather achronic, study. There is no universal history but a universal intelligibility. However, Démeunier is a man of the Enlightenment; he admits and desires the progress of 'civilization'. But he does not mean to designate by that word a privileged ensemble of usages toward which all societies tend or should tend. Each society can civilize itself but according to its own pattern; not by changing customs but by avoiding the abuses which arise at their very origin – human weaknesses with their progressive complications. In other words, every society can be judged, but it must be judged for itself, on its own terms. There exists no universal model for civilization. The unity of humanity therefore includes its differentiation. Anthropology – and Démeunier heralds it – always sees in man a variant of himself.

Thus, the bare outlines of structural anthropology were drawn or, at least, thanks to structural anthropology, we can now reinterpret the

5. 'We are not speaking of religion usages; we have omitted such research', Démeunier laconically writes in his preface, from discretion, no doubt.

past. How and why it has been constituted today is another question the study of which deserves another investigation. Then, perhaps, it would be less necessary to find out what prefigured it than what it fought against. Traditions, indeed, can appear either as a support or as an obstacle. In either case they are no less useful.

References

Gennep, A. van (1910), Un ethnographe oublié du XVIII[e] siècle, *Revue des Idées*. Paris.

— (1914), La méthode ethnographique au XVIII[e] siècle, reprinted in *Religions, moeurs et légendes. Essais d'ethnographie et de linguistique*, vol. 5, part 2. Paris, Mercure de France.

Leiris, Michel (1934), *L'Afrique fantôme*. Paris, Gallimard.

Lévi-Strauss, C. (1962a), J. J. Rousseau, fondateur des sciences de l'homme, *Jean-Jacques Rousseau*. Neuchâtel, Editions de la Baconnière.

— (1962b), *La Pensée sauvage*. Paris, Plon.

— (1960), *Leçon inaugurale*. Paris, Collège de France.

— (1955), *Tristes tropiques*. Paris, Plon.

— (1950), Introduction à l'oeuvre de Marcel Mauss, *Sociologie et anthropologie*. Paris, P. U. F.

Lienhardt, G. (1954), Modes of thoughts, *The institutions of primitive society: A series of broadcast talks*. Oxford.

Métraux, A. (1963), Les Précurseurs de l'ethnologie en France du XVI[e] au XVIII[e] siècle, *Cahiers d'histoire mondiale*. Neuchâtel, Editions de la Baconnière.

Montaigne, M. (1964), Des Cannibales, *Essais*, vol. 1, ch. 31. Paris, Gallimard (Bibliothèque de la Pléiade).

Montesquieu. C. (1951), *L'Esprit des lois*, vol. 2. Paris, Gallimard (Bibliothèque de la Pléiade).

Radcliffe-Brown, A. R. (1958), Precursors of social anthropology, reprinted in *Method in social anthropology. Selected essays*. M. N. Srinivas. (Ed.), Chicago, University of Chicago Press.

Rousseau, J. J. (1965), *Discours sur l'origine et les fondements de l'inégalité*. Paris, Gallimard.

— (no date), *Essai sur l'origine des langues*, ch. 8.

Tinland, Franck (1968), *L'Homme sauvage*. Paris, Payot.

3 Anthropological traditions: Their definition

BOB SCHOLTE

1. Syntagmatic and paradigmatic frameworks

What do we mean by the term 'anthropological traditions' and how do we study them? To begin with, such traditions refer to specific bodies of ethnographic data and ethnological theories. They are not further reducible, that is, they are defined solely by their syntagmatic position within the history and structure of anthropology. It is within this framework that the generic characteristics and universal conclusions of anthropological investigation are located, determined, assessed and improved.

The purpose of this essay is to challenge the premises and conclusions of a syntagmatic framework and to provide a paradigmatic alternative (Kuhn, 1962). From this perspective, anthropological traditions are not merely composed of facts and thoughts *about* society; they are also parts and reflections *of* society (Gouldner, 1970, p. 13). To study anthropological traditions paradigmatically is to determine their mediated status as cultural artifacts by subjecting them to ethnographic description and ethnological comparison. Their generic characteristics are thereby contextualized; their universal conclusions relativized. A uniform and cumulative framework is replaced by a radical and critical perspective in which judgment and progress are dialectically constituted rather than analytically posited.

The notion of such an *ethnology of ethnology* seems so deceptively obvious that it is surprising to find so few precedents in the anthropological literature. Though some anthropologists have called for such a perspective (e.g., Berreman, 1966, p. 350), until quite recently not

many have paid more than lip-service to 'the question of the degree to which our Western scientific treatment of socio-cultural data is itself a patterned and partial phenomena' (DuBois, 1963, p. 33). True, some have done decidedly more, among them Diamond (1963), Hallowell (1965), Maquet (1962), Smith (1964), White (1966) and especially Bennett (1946). But an intense concern with a reflexive, critical and dialectical anthropology is only a few years old. The contents of this volume (e.g., Krader's article), the introductory essay and the recent work of its editor (e.g., Diamond, 1964 a, 1971, 1972) the statements of some of those involved in the ethics debates (Berreman, Gough and Wolf) or the current efforts of anthropological minded sociologists (e.g., Lepenies, 1971 and Wolff, 1971) all bear witness to the recent appearance and practical urgency of a context-sensitive paradigm for the analysis and assessment of anthropological traditions. This fact is itself of interest to the ethnologist of ethnology and requires some explanation.

Why the relatively sudden change in perspective when both the syntagmatic framework and its paradigmatic alternative were widely known and had been hotly debated by historians, philosophers and sociologists since the early nineteenth century (Moscovici, 1966, p. 120)? I would suggest two complementary reasons: a syntagmatic point of view prevails when normal science (puzzle-solving) is conducted on the institutional basis of a shared consensus about the basic principles for a continuous history of anthropological progress (Kuhn, 1962, p. 10ff.). Conversely, a paradigmatic challenge appears in times of socio-intellectual crisis when such institutional agreement is shattered, that is, when the logical issues and empirical data central to disciplinary debate are no longer sufficient to sustain an ontological, epistemological and normative consensus. This is what has happened to cultural anthropology in the past few years.

There is currently a crisis of conscience among cultural anthropologists. Self-criticism has placed many of our efforts in doubt. Ethical differences have created a schism in the profession. We do not even agree on areas of disagreement! The specific issues raised are complex and diverse, but one general conclusion stands out: to the extent that the anthropological establishment is part and parcel of the history of Western culture, its ideological innocence and affective neutrality as a universal and objective science can no longer be taken for granted. It is and should be challenged on practical and theoretical grounds (e.g., Fabian, 1971; Gouldner, 1970; or Habermas 1971). Important from our point of view is the fact that

the syntagmatic reification of anthropological traditions is itself part of the problem, not a solution, or an escape. It is symptomatic of a cultural circumstance – the fetishization of scientific logic and operational techniques (Lefebvre, 1966, p. 282). As a framework for the analysis and evaluation of anthropological traditions, a syntagmatic stance is partial (in every sense of the term): it reifies the *texts* of ethno*logical* systems at the expense of understanding the *con*texts of *ethno*logical productions. The recent turn to a paradigmatic perspective is a return to first principles: the ethnographic description and the ethnological comparison of anthropological traditions as the products of a human *praxis* and the results of a presupposed *Lebenswelt*.

2. The principles of context and comparison

A syntagmatic viewpoint is partially correct: 'to some degree, the elaboration of (an anthropological) theory has a life of its own; technical concerns provide it with a measure of autonomy' (Gouldner, 1970, p. 396). It is partial and false if it abstracts these indigenous concerns from their paradigmatic contexts. An anthropological theory is also embedded in and shaped by a variety of other potent forces: 'the sentiments, the domain assumptions, the conceptions of reality accentuated by personal experience, all these constitute its individual and social grounding' (Gouldner, 1970, pp. 396–397). Ideological self-deception generates an illusionary preference for the syntagmatic: 'as the light dove might imagine that it would fly better in a vacuum, the thinking individual likes to dream of thinking free from [the] unthought deposit that has formed within him, under the rod of his mentors, and which underlies all his thought' (Bourdieu, 1967, p. 358). This ingrained and 'taken-for-granted' reality constitutes the cultural context of anthropological traditions. By comparison, the syntagmatic structure of ethnological thought is but a hollow abstraction and a delimited version of the more encompassing paradigm from which it derives and in which it remains anchored.

A paradigmatic understanding of anthropological traditions should, then, precede their comparison and evaluation. Let me first divide the ethnographic setting of ethnological thought into four interrelated and complementary domains and give some 'context-sensitive' and comparative examples for each.

2.1 *A note on priorities*

The four contextual domains I would use as my paradigmatic framework are the existential, the social, the historical and the philosophical. The first of these – the existential – is especially difficult to 'locate'. Some people would argue that the existential domain constitutes the experiential 'habitat' of the three other domains, especially in the case of ethnographic *praxis*. When one reads, say, Bowen's *Return to laughter* (1964), Castaneda's *The teachings of Don Juan* (1968), Malinowski's *A diary in the strict sense of the term* (1967) or even Lévi-Strauss' *Tristes tropiques* (1955), one is inclined to agree with Geertz: 'All ethnography is part philosophy, and a good deal of the rest is confession' (Geertz, 1967, p. 25). On the other hand, confessions may be further reducible and objectifiable. This is what Lévi-Strauss (1963) would have us believe about Rousseau's *Confessions*. But one would then have to argue that the existential domain is culturally mediated and that it is therefore derivative in comparison to sociological, historical or philosophical contexts (for Lévi-Strauss, of course, a paradigmatic framework is in turn reducible to the inherent structure of the unconscious brain; see Scholte, 1972a). For our purposes it is perhaps sufficient to point out that in *praxis* and *in situ* the relation between the biographical and the cultural is a dialectical one. In an ethnographic situation, the existential dimension tends to be accentuated and dramatized; in an ethnological analysis, suppressed or neutralized.

There is another difficulty in discussing the existential domain. The actual information available to us is often quite partial. So is its subsequent use. Not all 'confessions' are equally thorough or illuminating. Some may be motivated by idiosyncratic passions. Others may be distorted by the censorship of the super-ego. Similarly, the use of personal material may be caused by irrational and destructive motives. Or it can be inhibited by a judicious respect for personal privacy. As a result, existential information is usually reserved for heated debates, discussions in the back corridors of academia, the obituary columns of departed colleagues or the historical analysis of 'dead' pioneers (Helm, 1966).

There is yet another consideration. Biographically relevant information reaches the observer-analyst in the form of personal conversations or written documents, that is, as 'objectifications of subjectivity' (Fabian's terms). These 'mediated' texts rather than the personal realities them-

selves carry 'abridged' information about the existential domain. Since such texts are in turn motivated by an anthropological purpose and rarely by a merely personal one ('my *field* experience', not simply 'my experience'), the ethnographic description of biographical materials and their subsequent analysis cannot be equated with psychoanalytic interpretation or with psychological explanation. The proper understanding of existential information in *a paradigmatic framework for the study and evaluation of anthropological traditions* is always one step removed from the psychological goal of explaining the subjective experience of the anthropologist in a syntagmatic framework.

The ethnologist of anthropological traditions is interested in the specific function of personal experience in mediating *interpersonal* understanding–whether between ethnographer and 'native' or between one ethnologist and another. He is *not* interested in reducing the reflexive study of anthropological traditions to a mere branch of psychology (or sociology, history, philosophy). Nor is he interested in dissolving anthropological *praxis* into a mere variant of psychodynamic activity. An anthropologic is always an ecologic, but never an *ego*-logic. Given the American (and hence culturally mediated) bias toward psychological explanations (e.g., Lévi-Strauss, 1946), this explanatory priority must be kept in mind at all times.

2.2 *The existential domain*

The existential mediation of anthropological knowledge is most concretely evidenced by ethnographic *praxis*. There is an irreducible uniqueness to each anthropologist's field experience by definition. For one, we all have our biological differences and our psychological idiosyncracies. So, of course, do our native informants. Such differences and idiosyncracies may or may not be important from a strictly ethnological point of view. Often they are, as in the case of biological gender. Men and women anthropologists tend to have disparate access to ethnographic information (e.g., Gonzales, 1970, or Urbanowicz, 1970). Psychological 'residues' may also be significant. If, for instance, one wanted to assess the functional relationship between Malinowski's *Diary* and his *Argonauts*, psychological variables would certainly have to be taken into account (even if they were subsequently dismissed). Psychological factors may be equally important to the proper understanding of native informants. They are persons as well as informants, and their individuality

and integrity are a distinctive feature of any ethnographic situation. If anthropologists are 'marginal natives' (Freilich, 1970), so are most native informants. The degree of their 'indigenality' or 'marginality' may be a decisive element in determining what we know or have been led to believe about native societies (e.g., Berreman, 1962, or Diamond, 1964 b).

The existential domain may also serve a vital *epistemological* function in the field itself. A biographical context would then be of more than idiosyncratic or residual interest. It would, in the first instance, mold interpersonal communication and the subsequent formulation of intercultural comparison. For example, in ethnographic *praxis*, the existential domain 'inhabited' by the anthropologist and the field situation he enters are co-terminus. The ethnographer is literally and bodily *in* the field. What he finds there *and* what he brings with him define that field situation (and no other) as a concrete totality for him. This totality entails the existential, social, historical and philosophical 'pre-understanding' *(Vorverständnis)* of the native *and* the anthropologist. Field work, in other words, is defined by the nature of encountered phenomena *and* by the nature of the encounter. To that extent ethnographic experience and existential reality are continuous. This also means that the result of ethnographic activity is always a mediated and conditioned achievement. It is a function of a dialectical and generic process: the constitutive relation between ethnographic producer, ethnographic production and ethnographic product (Fabian, 1971, p. 27). Ethnographic performance is a context-sensitive dialogue between two dissimilar ethnologics in which intentionally motivated and concretely tangible people are trying to communicate and to understand each other's paradigmatic frameworks. The resultant ethnography is never 'a solitary abstractive function of a (scientifically competent) mind imagined to operate outside of space, time and the contexts of other minds' (Fabian, 1971, p. 25).

There are two different ways of evaluating the epistemological significance of the experiential continuity between biographic context and ethnographic reality. The practicing anthropologist can try to maximize or to minimize its effect. If he decides on the latter course, he will attempt to 'neutralize' the existential mediation of ethnographic knowledge. If he cannot erradicate it, he can at least try to hold its effect 'constant'. He can try, for example, to subsume the personal immediacy of his ethnographic experience under the scientific rubric of a super-

individual logic. There are descriptive and analytic techniques for doing so – for 'testing', 'purging', 'objectifying' ethnographic *praxis* and its resultant product (see Scholte, 1971b). He can even attempt to do so without or before entering the field. Lévi-Strauss calls this experimenting on a model; others call it having a problem-oriented research proposal. Either way, the existential province is reduced to the status of a residual category. It is assumed to be *discontinuous* with, or at least derivative of, the superorganic reality of scientific traditions.

The cultural anthropologist can also cultivate an explicit awareness of the experiential continuity between biographical context and ethnographic reality. If he takes this approach, he will deliberately 'enter into' the ethnographic process of communicative interaction. He will attempt to 'surrender' to its interpersonal immediacy (Wolff, 1964, p. 237ff.). He can try, for example, to involve himself in a common discourse with his fellow men *and*, at the same time, try to explicate – as part of his ethnographic understanding – the paradigmatic assumptions that make his anthropological activity possible in the first place. There are dialectical and hermeneutical principles for doing so – for explicating, concretizing and contextualizing a specific experience and its intersubjective results. This, too, he can do before or without entering the field. I would call this the reflexive study of anthropological traditions; others might call it an *engagé* account of anthropology's personal and humanistic relevance. In either case, the existential province is included in a total epistemological setting. It is shown to be continuous with, or at least a part of, a paradigmatic framework for anthropological science.

There are personal, social, historical and philosophical reasons for prefering the second alternative, that is, for insisting on a paradigmatic framework for anthropological *praxis*. There are, first of all, psychological advantages to recognizing that ethnographic knowledge is existentially mediated. Including the biographical setting gives the 'anthropological imagination' (see Mills, 1959, p. 6ff.) its due. The personal relevance of understanding interpersonal reality would be continuous with, and complementary to, its ethnological relevance. The ethnographer would no longer be asked to turn himself into an addendum to scientific logic. Ethnographic detachment and ethnologic reification would be understood for what they are: syntagmatic reifications of human *praxis* (Monod, 1970). Conversely, an explicit recognition of the continuity

between experience and reality would preclude an inverse mystification–the reduction of ethnographic experience to mere psychodynamic indulgence. An 'awareness of textured reality enables "variables" to be seen as *self*-experience and allows them to be mobilized for *self*-understanding' (Gouldner, 1970, p. 56). The ethnographer would be in a position to assess the degree to which he is, in fact, imposing idiosyncratic and irrelevant judgments on intersubjective experience. A paradigmatic understanding of the dialectical relation between self and other, experience and reality, *praxis* and theory, is in this sense tantamount to a *diacritical* understanding of the constitutive function of the existential province.

There are related epistemological advantages to consider as well. For example, if ethnographic knowledge is existentially mediated, the synthetic achievement of the knowing subject must be recognized. Since an anthropological *praxis* is always a human *praxis*, ethnographic understanding is never a mere question of syntagmatic competence or supra-individual virtuosity. It invariably presupposes a paradigmatically mediated performance by an existentially situated ethnographer. As one human being among others, the anthropologist can only 'penetrate into alien expressions of life through a transposition from the fullness of [his] own experiences' (Dilthey quoted by Habermas, 1971, p. 114). The intentional structure of ethnographic consciousness is the epistemological prerequisite for the concrete recognition of one's fellow human beings and the subsequent description of their life-styles. Theoretical discontinuities and metalinguistic devices can only be introduced after the fact and in the abstract. As Vico initially pointed out, the existential immediacy of mediated knowledge in the human sciences is an asset, not a detriment: 'real comprehension presupposes the utmost liveness of the comprehending subject and the richest unfolding of his individuality. Just as the interpretation of a work of poetry and of art can only be a success for those who allow themselves to be reached by it, so the comprehension of an [anthropological] or sociological text can only be such for those who are stirred by the problems of [cultural] and social life' (Bultmann, 1955, pp. 255–256).

The existential domain, as a *precondition* to anthropological *praxis*, is irreducible. It can and should be related to other paradigmatic factors, but it cannot and should not be subsumed under the syntagmatic rubric of scientific discourse. Whatever objective and comparative understanding we can hope to gain is ultimately a dialectical function of anthro-

pology's epistemological and contextual setting as a whole. Ethnographic descriptions are never mere static particulars that can be analytically reduced to illustrative and derivative instances of ethnological laws and principles. They are always the result of a sympathetic identification, a dynamic process, and a creative resolution (see Fabian, 1971, p. 25ff.).

2.3 *The social domain*

In emphasizing the existential domain's fundamental role in interpersonal communication, one is eventually led to ponder questions proper to the social domain. For 'Whoever says "Man," says "Language," and whoever says "Language," says "Society"' (Lévi-Strauss, 1955, p. 389). There are, of course, many other contextual factors that can be considered integral to the sociocultural mediation of anthropological traditions, but 'ethno-linguistic' factors are certainly among the most important.

One long-neglected area in this connection is literary style (see Hyman, 1962). To what extent do literary style and aesthetic preferences effect description and analysis? In Lévi-Strauss' case, for example, one is struck by the metalic and pristine, yet intricate and baroque, quality of his ethnographic descriptions. His ethnological analyses, in turn, are characterized by an overriding interest in the mineralogical and the stable (Bataille, 1956) while his recent *Mythologiques* express an infinite concern with the intricate variations and 'bizarre' (if inherently logical) involutions on an underlying theme (see Scholte, 1969a).

Beyond literary style, there are other questions associated, for example, with the application of the Sapir-Whorf hypothesis to social scientific discourse (e.g., Bourdieu, 1967, p. 345). One might designate this as a problem in the relation between habitual patterns of ethnological behavior and grammatical categories of syntagmatic reification.

Since scientific discourse, like any other, is also an ethno-linguistic phenomenon, ethnographic descriptions and ethnological analyses are mediat*ed* as well as mediat*ing* activities. They are never conceptually neutral or isomorphic with anthropological 'reality'. They are 'symbolic forms' in Cassirer's sense of the term. Their access to 'objective reality' is therefore always one of 'indirect directness'. Since they are constitutive of reality, 'there is no discovering ... virginal meanings in the *rite de passage* of explication' (Duerr in *Current anthropology*, 1970, 11, 1: 74).

As situated realities, they are in addition subject to 'ethnocentricities of meaning' (Mills, 1963, p. 435).

Professional idioms of anthropology provide excellent examples of such 'tainted' ethnocentricities. Though they are presumably designed to 'transcend' situational vicissitudes, they are in fact exemplary of an academic paradigm which generates a syntagmatic bias and which rewards an illusory competence. In the anthropological writings that result, the realities of existential involvement, ethnographic surrender and communicative interaction are quite literally co-opted by the artificial textual restrictions of an objectified, bifurcated and analytic metadiscourse. The logical syntax selected creates a descriptive *stasis* that necessarily violates the flow of ethnographic experience and inhibits the reflexive moment in anthropological understanding. Separating self from other thus becomes a circular, self-fulfilling process.

Abstract ethno-*logical* metalanguages are concretely and *socio*-logically generated. Professional jargon is more than merely symptomatic of 'terminological escapism' (Myrdal, 1969, p. 57). It also reflects the 'formal and empty ingenuity' (Mills, 1959, p. 78–79) of those academic mannerisms that equate scientific competence with a syntagmatic framework. The social institution that sustains, nourishes and rewards this irrational and reified competence has aptly been termed a 'pedantocracy' (Bakunin; see also Hellenes, 1970, and Gouldner, 1970, p. 120ff.).

Many cultural anthropologists are members of – even apologists for – these bureaucratic 'pedantocracies'. Little wonder that they remain largely ignorant of the ethnological paradox entailed in a syntagmatic standpoint. As ethno-scientists they presumably realize that 'criteria of Logic are not a direct gift of God, but arise out of, and are only intelligible in the context of, ways of living or modes of social life' (Winch, 1958, p. 100). As professional scientists, however, they operate as if their ethno-logics were autonomous, as if the *ethno*-logical confines of their mediated *praxis* had for some magical reasons become inoperative in their particular case. The assumption that the anthropologist 'is free from the very social pressures whose importance he affirms when thinking about other men' (Gouldner, 1970, p. 54) is tantamount to a false-consciousness which is both dangerous and inexcusable.

There are, of course, any number of other contextual factors involved in the sociocultural mediation of anthropological traditions. These can only be determined on an *a posteriori* basis for each specific case. But

one factor is implicit in all instances, namely, 'the social organization of ethnological theory' itself. It is curious that many anthropologists – while devoting their professional lives to the comparative study of social organizations – should *not* have made the sociocultural domain an equally integral part of their paradigmatic studies of anthropological traditions. In the majority of 'histories of anthropology' there are few if any succinct and detailed references to the *sociology* of ethnological knowledge, e.g., to anthropological 'schools', academic institutions and their geographical locations, research organizations and funding agencies, professional elites and graduate students, etc. This despite the rather obvious idea that 'if society is in anthropology, anthropology is itself in society' (Lévi-Strauss, 1960, p. 51). As a result, one cannot expect to write an adequate history of anthropological theory without, at one and the same time, writing an ethnography and an ethnology of that history.

There is, of course, a positive, or better 'functional', side to the sociological coin. Without some form of financial and organizational support there would be no 'leisure of the theory class' and hence no continuous intellectual tradition. But it is precisely here that the possibility of distortion becomes an especially prominent concern for the so-called malcontents in anthropology – those who have lost faith in the discipline's syntagmatic stature and ideological innocence.

The prevalence and timeliness of recent critiques of the anthropological 'establishment' provide important examples of this paradigmatic consciousness. These critiques are not motivated exclusively by a political purpose (though they are that, too, and rightly so). They also have a more inclusive theme in common: If 'the social organization of intellectual activity is significantly related to the character of the knowledge which develops under its auspices' (Merton, 1957, pp. 485–486), scientific *praxis* is affected by social conditions *and* vice versa. To separate the two, as in the case of the syntagmatic reification of scientific discourse, inhibits epistemological awareness ('the assessment of the "ethnological" assumptions entailed in the possibility and constitution of any anthropological knowledge whatsoever' (Scholte, 1971b, p. 2) *and* contributes to the false-consciousness of applied scientism (reflecting and resulting in 'a technocratic, bureaucratic, and conservative *praxis*', Scholte, 1971a, p. 790).

Adopting a paradigmatic framework in studying 'the social organization of ethnological theory' is like opening Pandora's box. There are

personal, political, moral and philosophical questions involved of enormous theoretical proportions and of immediate practical concern. Take, for example, graduate training in anthropology. A young scholar entering graduate school may assume that there is, or should be, a *de facto* continuity between the existential meaning of his personal life and the sociophilosophical value of his anthropological contribution. He assumes, in other words, that wisdom and scholarship have something in common and that 'thought is at one and the same time being and knowledge of being' (Laing and Cooper, 1964, p. 40–41). But this assumption is rarely if ever shared by the institution which the student initially enters or by the one in which he or she may someday work. As far as graduate training is concerned, a department is more interested in 'programming' the student into 'correct' patterns of academic behavior and 'like-minded' patterns of scholarly thought. He is molded into 'the "right" mode of intellectual activity' (Bourdieu, 1967, p. 350), that is, into a distinctive and partial way of *learning how to learn* (see Bateson, 1972). It is precisely because anthropological knowledge is acquired in the institutional context of metalevel learning that the syntagmatic illusion of scientific transcendence can be created and perpetuated.

The organized process of graduate training is also a *socio*-logical one. The student's intellectual success is socially marked and socially motivated via the initial *rite de passage* of the doctoral examination and the subsequent acquisition of the 'union card'. A successful performance at the initiation ceremony is awarded with an institutional certificate of professional competence. The Ph. D. degree defines the anthropologist's membership in the 'cultural *habitus*' (Bourdieu, 1967, p. 344) of academic society. The circle is complete; personally felt motives and existentially-mediated values have been 'purged', 'socialized' and 'objectified' by the syntagmatic sieve of graduate education and academic professionalism. The anthropologist is now a full-fledged scientist, that is, a split personality (see Diamond, 1971, p. 3). In his newly acquired role as the alienated and 'relativistic professional' (Diamond, 1971, p. 3), he is unlikely to ever recapture a personal sense of existential reconciliation. The latter can only result from a scientific and humane *praxis* that sustains the immediacy of a dialectical continuity between lived experience and anthropological reality. Institutional anthropology bears little relationship to this 'anthropology of experience' (Berreman, 1971, p. 6). Scientific objectivity, syntagmatic competence, sub-specialized techniques and bureaucratic trivialization are inimical to 'the existential

realities of human lives' – the anthropologist's own or any other (Berreman, 1971, p. 6; see also Scholte, 1971a, p. 783ff.).

What is so tragic and ironic is that this price has been paid for a mere illusion. Graduate students know that their professional training and specialized skills are neither neutral nor objective. They are political and normative, in fact or by implication (see Paulson, 1970, p. 6). Many of them also know that some of their professors, conservative and radical, behave like tigers in the classroom but like pussy-cats in the Dean's office (see Gouldner, 1970, p. 411). More and more students are becoming knowledgeable about the pretensions of many university administrations (see Berreman, 1971, p. 7) and are learning that their graduate careers are by and large an institutional process of alienation and dehumanization.

The syntagmatic reification that results from such graduate training is, of course, ideologically and paradigmatically mediated. An anthropology 'with a non-partisan image can become instutionalized when the elites of a society are confident that its social scientists are, in fact, *not* neutral' (Gouldner, 1970, p. 470). A universalistic and professional ideology cannot hide the economic and political fact that 'the normative pattern of science by and large reflects the expectations of the power-holders' (Schmid, 1968, p. 63). What is even more regrettable is that too many of us feel academically conditioned to accept our false-consciousness and economic dependence (see Gouldner, 1970, p. 440ff.). In the face of the 'military-industrial complex', the acquiescent professional merely shows 'an increasing concern for pure technique' and often descends into 'triviality and irrelevance' (Wolf, 1969, p. 9).

How does the formal rationality of syntagmatic discourse respond to such critically motivated and paradigmatically founded misgivings? By 'the sanctimonious and reactionary demand that the politics of dissent be removed from the ivory tower so that the truth can be sought and taught without distraction or interpretation – in the naive belief or cynical assertion that there is a truth independent of the human reality within which it exists' (Berreman, 1971, pp. 6–7). Perhaps such pristine convictions provide the cultural anthropologist with an academic job but never with a 'moral center' (Diamond, 1971, p. 26). It does, of course, help to explain the ironic and immoral history of an anthropological scientism that is 'an instrument ... of imperial civilization' (Diamond, 1971, p. 26) which has largely neglected the comparative study of Western colonialism (Gough, 1968, p. 20). Only a paradigmatic

and diacritical perspective can expose and evaluate the social genesis and moral implications of this or any other aspect of the syntagmatic illusion. The normative and emancipatory question of alternative and context-sensitive performances can and should then be raised (see Scholte, 1971a, p. 798ff; 1971b). If we do not do so, an allegedly comparative anthropology would in fact remain an echo of the partial ethno-logics peculiar to its own Western environment (see Diamond, 1971).

The social organization of ethnological training is but one of many factors that could be operative in the case of a specific anthropological tradition. As I have stressed throughout, paradigmatic elements are context-sensitive and context specific. They cannot therefore be posited *a priori*, though they are usually determinable *a posteriori*. As part of the social 'environs' of French structuralism, for instance, one would have to consider a diversity of factors ranging from the demographic to the religious (see Scholte, 1970).

Since such examples can be multiplied *ad infinitum*, let me close this section on the social domain on a comparative note. A comparative perspective is an integral part of any anthropological discourse – the ethnology of ethnology included. It can be applied diachronically and synchronically, with the result that one arrives at an obvious, albeit important, conclusion; namely, anthropological traditions differ, not only historically but culturally as well. There are, in other words, 'ethnocentric orientations' (Smith, 1964, p. 259) to differing anthropological traditions. One has to content with the 'national character of the social sciences' (Aron, 1939, p. 113). These national and cultural influences are not only evident in extreme political instances such as the racist anthropology of fascist states (see Lips, 1966) or the reactionary ethnology of imperialist countries (see Gough, 1968). They may also be operative in most, if not all, instances. One is thus 'led to reflect on the role that social science plays in national ideologies and the ways in which the current state and development of social science reflects other cultural states and processes' (Halpern and Hammel, 1969, p. 17). Like any other paradigmatic framework, such cultural comparisons entail an explicit critique of the syntagmatic point of view: 'One sees that it is much more appropriate to disgard old notions of the distinction between "science" and "folklore" and to regard the social science of a particular society, however sophisticated and presumably objective, as an important part of its subjective ideology about itself and the world

and thus a part of its folk theory about the relation of man to society and of men to men' (Halpern and Hammel, 1969, p. 17).

The sociological implications of ethnocentrically mediated traditions are especially clear in cases of debates between mutually exclusive positions. Here a comparative perspective is the essential precondition to a proper understanding of the sociocultural – and hence intellectual – issues involved. But more often than not, we forget to distinguish between debates among 'socially and intellectually homogeneous participants' and those among 'socially and intellectually heterogeneous participants' (Mannheim, 1939, p. 279; see also Kuhn, 1962, pp. 108–109). In the latter case, an awareness of the cultural context involved may be as important as an understanding of the substantive content. If the antagonists concerned overlook these contextual variables, they will almost invariably end up by talking past one another rather than at the issue. Says Mannheim (1939, p. 280): 'although they are more or less aware that the person with whom they are discussing the matter represents another group, and that it is likely that his mental structure as a whole is often quite different when a concrete thing is being discussed, they speak as if their differences were confined to the specific question at issue around which their present diagreement crystalized. They overlook the fact that their antagonist differs from them in his whole outlook, and not merely in his opinion about the point under discussion.'

Most anthropologists are aware of the dangers of ethnocentric translations from one ethno-semantic domain into another. In fact, they strenuously seek to avoid false descriptive impositions or culture-bound theories. They do not, however, always extend the same courtesy to their fellow anthropologists or to their historical predecessors. If the historical context is overlooked, the result is 'presentism', that is, studying 'the past for the sake of the present' (Stocking, 1965a, p. 211). If competing schools of thought are involved, history is either forgotten (see Krader, 1968a) or becomes just another battle ground. In current systematic debates a neglect of context may also lead to misunderstanding and misrepresentation. The largely misguided polemics involved in the empirical critiques of structuralism (see Scholte, 1966, or 1972b) and of transformationalism (see Grace, 1969, or Nutini, 1968) constitute concrete examples. Here as elsewhere, a paradigmatic perspective could have prevented ethnocentric conclusions and would, furthermore, have provided more telling critiques and more adequate

assessments (see Scholte, 1972a, for structuralism and Hymes, no date, for transformationalism).

2.4 *The historical domain*

The anthropological importance of the historical domain is twofold. First of all, the current state of anthropological inquiry is in part derived from, and understood in terms of, the historical development of the discipline as a whole. Even a syntagmatic framework posits a historical trajectory in which anthropological science is located and through which it advances. As previously noted, however, this approach remains a reified one. It tends to view the history of anthropology as a 'chronology of founding fathers suspended in an unmotivated social and historical vacuum' (Darnell, 1971, p. 84). To rectify the syntagmatic point of view, a context-sensitive and properly paradigmatic standpoint is required. Second, then, the anthropological importance of the historical domain is also defined by the complementary function of providing an *ethno*-logical dimension for chrono-*logical* studies. The anthropological *and* historical advantage of doing so is enormous: 'the history of anthropology considered as an anthropological problem supplements an exclusive concern with the history of organized inquiries and any attempt to arbitrarily isolate the development from its roots in a wider cultural context. On the contrary, it directs attention to the cultural contexts and historical circumstances out of which [intentionally structured] formulations of anthropological questions must have developed ...' (Hallowell, 1965, p. 24). A problem in the history of anthropology is also a problem in the anthropology of history and *vice versa.*

The dialectical relation between historical context and anthropological *praxis* is dramatically illustrated by the cultural genesis of anthropological consciousness itself *and* by its correlated diacritical function in Western civilization. Burridge, in a recent and as yet unpublished essay, shows why and how a 'scientific' interst in ethnographic description and cross-cultural comparison arose exclusively in the West as a distinctive response to the Occidental (initially Renaissance) problem of the dichotomous and tensional relation between self and other, self and society, one society and another. The Western preoccupation with self-analysis and sociocultural explanation generates a specific mentality which 'assumes not simply a tradition of interest in others, but a built-in awareness of otherness which leaps forward to comprehend and appropriate different

kinds of otherness' (Burridge, no date, p. 15). That such comprehension *qua* appropriation must have reflected back on the European anthropological tradition seems certain, though the relationship is probably a complex one (e.g., the intricate if obvious ties that have and continue to exist between anthropological *praxis*, imperialist expansion, political control and scientific predictability). The Western genesis of cultural anthropology and its self-conscious interest in comparative understanding as a scientific process must have in turn affected the historical development and intellectual continuity of the discipline. For example, ethnographic information – whether in the early form of travellers' accounts and imaginary voyages or in the recent form of 'participant' observation and 'controlled' description – must have accentuated European hopes for a comparative science of social reality. Such aspirations, then, simply furthered the continuous importance of ethnographic activity, ethnological analysis and objective inquiry.

The historical origin and subsequent development of cultural anthropology in Western society and its generic and continuous role in providing a diacritical perspective on European problems are complementary. As Derrida (1967, p. 4143 my translation) points out 'ethnology could only have been born as a science at a time when a decentralization could be effected; at a moment when European culture – and consequently the history of metaphysics and its concepts – was dislocated, expelled from its rank, and thus ceased to be considered as a culture of reference. That occasion [was] not primarily a moment in philosophical or scientific discourse, it [was] also a political economic, technical, etc. event.... There is nothing fortuitous about the historical and systematic contemporaneity of critical ethnology and the destruction of the history of metaphysics. Both pertain to one and the same epoch' (see also Wolff, 1959). In a more contemporary context, cultural anthropology may be defined as 'the search for man in history, undertaken by a society threatened by automatism' (Diamond, 1963, p. 6; see also Diamond, 1971). In any event, both ethnography and ethnology are historically mediated *and* historically motivated phenomena.

The dialectic between anthropology's ethnogenesis and its ethnocritical role has a liberating and an ironic aspect. The irony lies in the very separation between self and other and the attendant appropriation of otherness *qua* 'object' of scientific study. For one, 'the objectification of the other is necessarily irrational, since rational justification presupposes the recognition of the Other as subject' (Hellesnes, 1970,

p. 44). For another, objectification generates reifications, such as the concept of 'Man' or the reduction of men and women into 'data' and 'things'. The results are paradoxical. The existential, social, historical and ideological conditions 'which have made possible the concept of mankind as an abstract, unitary, objective totality [also] impede the [concrete] realization of the concept' (Krader, 1966, p. 52; see also Krader, 1967). What Stanley Diamond rightly calls 'the search for the primitive' is not only a symptom of cultural anthropology's historical genesis but also an indication of its sociocultural intent. Threatened by automatism and alienation, Occidental anthropology again seeks, as it originally sought, to understand an existential, personal, nominal and individuated 'primitivism' which the essentialistic, abstract and mechanical collectivism of Western civilization finds increasingly detrimental (Diamond, 1963, p. 54ff.). Western anthropologists seem to realize that 'primitive peoples understand the preconditions of a human existence' (Diamond, 1964 b, p. 130) with a kind of immanent authenticity which they do not. Ironically, however, most anthropologists turn to the normative task of cultural understanding and comparative critique with the analytic techniques, impersonal categories and objectivistic aims which sustained their civilization and created their science – that is, reifications – in the first place!

The only solution, at least in the specific context of the historical domain, is to radicalize the diacritical function of anthropological *praxis* to its logical and reflexive conclusion, that is, to describe, analyze and evaluate the genesis and implication of *our* mediated ethnological perspectives. This would entail both a context-sensitive and a critical understanding of the history of anthropology – including its generic and intentional structure. This means, for example, that 'becoming conscious of a tradition involves finding a historical heritage, but accepting the latter only as a function of criteria that are our own' (Pouillon, 1968, p. 9, my translation). The search for a historical context or tradition is, in this sense, itself a mediated, reflexive and diacritical effort. It generates a comparative perspective on *our* ethno-logic while we search for an ethnographic and ethnological understanding of *other* life-styles and traditions. It thereby also provides us with a critical, if mediated, criterion for differentiation and progress. To quote Pouillon once again, 'a new theory is constituted, not as a result of a previous and analogous theory which contains the original in embryonic form, but as an answer to an unresolved problem, or, more often to one not a yet posed'. In other words, 'traditions are discriminatory: that is their essential and

practical function... . Recourse to a tradition is a means of formulating its distinctiveness' (Pouillon, 1968, pp. 11–12). Differentiation, that is, comparison and contrast, is a result of understanding and a vehicle for critique, evaluation and (partial) transcendence. This is the liberating function of the study *of* the history of anthropology *in* history and of the paradigmatic understanding of the historicity of anthropological *praxis*. By comparison, the reification of an anthropological text at the expense of its historical context is illusory and binding: 'to think that one has managed to distance oneself from history may be a trick of the unconscious' (Pouillon, 1968, p. 7) akin to Hegel's 'cunning of reason'.

As in the case of the other paradigmatic domains thus far discussed, I have singled out only a few of many possible issues. As far as the historical province is concerned, the comparative question, in both its diachronic and geographic aspects, seems an especially important one. In terms of the history of anthropology as a Western phenomenon, for example, it 'is always a disservice ... to fix casually upon any handful of recent scholars as 'founders' or 'originators' (Hodgen, 1964, p. 7). Since anthropological inquiry has frequently broadened the restrictive space and time span of European intellectual history, I see no reason to assume that it cannot do so on behalf of its own proper circumstances and history as well. This would require the development of a genuinely cross-cultural perspective on the history and the geography of anthropological issues. True, sociohistorical conditions in the West may have been particularly favorable to the origins and continuity of scientific anthropology as we know it (Hallowell, 1965). Still, since the humanistic concerns of the social sciences have been the subject of serious reflection since the dawn of the human intellect, there is no reason to assume that a truly comparative anthropology cannot be devised in both historical and systematic terms. This would have at least one possible and liberating consequence: we might no longer simply co-opt non-Western anthropologists in the historical and systematic canons of our own ethnoscience.

I have given my discussion of the historical domain a distinctly 'Europo-centric' orientation. This is justifiable if we remember that the 'metacritical' radicalization of the historical context is itself a situated phenomenon. It, too, is diachronically mediated and historically motivated. I already mentioned this in the case of contemporary anthropology's changing circumstances and the attendant requirement of an

encompassing paradigmatic, rather than a merely syntagmatic, framework. I should add that the circularity implied here is inescapable: 'critical consciousness itself emerges only from the history of the development of consciousness' (Habermas, 1971, p. 17; see also Bennett, 1946, p. 374; Fabian, 1971; Kuhn, 1962; or Mills, 1959, p. 130ff.). In fact, the point is not so much to avoid the circularity as to get properly into it (Radnitzsky, 1968, p. 35), for that is what 'unites us to the totality of human experience no less than that which separates us from it' (Merleau-Ponty, 1951, p. 501).[1]

2.5 *The philosophical domain*

The philosophical context of anthropological *praxis* has never been a major concern of ethnographers and ethnologists (at least in this country). True, many prominant anthropologists have devoted considerable time and energy to the comparative study of ethno-philosophy (e.g., Albert and Kluckhohn's 1959 bibliographic survey). It is also true that general issues in ethnographic method and ethnological theory have received periodic and careful attention (e.g., Bidney, 1953, or Manners and Kaplan, 1968). Still, the kind of context-sensitive appraisals that have been made of non-Western epistemological, ontological and normative assumptions have not been made of the ethno-philosophical premises entailed in our own culturally mediated science of culture. In fact, the nature of the concern that has been expressed bears witness to a general

1. Merleau-Ponty's (1951, p. 501) sensitive appraisal of the relation between anthropological understanding and historical mediation reads as follows: 'If history envelops us all, it is up to us to understand that whatever we can have of the truth is not to be obtained in spite of our historical situation but because of it. Considered superficially, history destroys all truth, though considered radically it founds a new idea of truth. As long as I hold the ideal of an absolute spectator before me, of knowledge without a point of view, I can see my situation only as a principle of error. But having once recognized that through this situation I have become part of all action and all knowledge that can be meaningful for me, and that it contains, in gradually widening horizons, all that can *be* for me, then my contact with the social in the finitude of my situation reveals itself as the origin of all truth, including that of science; and since we have an idea of truth, since we are in the truth and cannot escape it, then the only thing left for us to do is to define a truth within a situation. Knowledge will be founded upon the irrefutable fact that we are not in the situation as is the object in objective space, and that it is for us the principle of our curiosity, our research and interest in other situations as variants of ours, and in our own lives, illuminated by fellowmen, as variants of the lives of others. Finally it is that which unites us to the totality of human experience no less than that which separates us from it.'

neglect of anthropological context. Philosophical inquiry into anthropological science are usually textual and syntagmatic, not contextual and pradigmatic. They may, for example, be attentive to 'explicit statements of scientific law [or] about scientific concepts and theories' regarding procedure and verification (Kuhn, 1962, p. 40), but they are rarely if ever concerned with more fundamental and more implicit assumptions – such as the epistemological premise that the universe is orderly, predictable and investigatible at all or the ontological alternative of considering men as rational or irrational, good or evil, free or determined beings, etc. This *ethno*-philosophic oversight is due to a syntagmatic neglect of the paradigmatic contexts in which first principles are generated and from which they draw their infrastructural sustenance. The syntagmatic illusion separates scientific competence from human performance and thereby overlooks both the paradigmatic contexts of philosophical principles and their diacritical function in antropological *praxis*. Not surprisingly, such a reified anthropology 'not only refrains from questioning its first premises, it also actively resists efforts to do so' (Murphy, 1971, p. 37). The counterclaim that 'metaphysics' is, or should be, irrelevant to scientific analysis only bespeaks this kind of anthropology's scientific and intellectual imperialism (see Scholte 1971a: 790ff.). From a paradigmatic point of view, a proponent of the syntagmatic framework is like Mannheim's (1936, p. 101) deluded positivist, a man who 'glories in his refusal to go beyond the specialized observation dictated by the tradition of his discipline, be it ever so inclusive, [and who] is making a virtue out of a defense mechanism which insures him against questioning his presuppositions'.

Philosophical assumptions in anthropological *praxis* may be general or specific, latent or manifest; their implications for ethnography and ethnology may be decisive or problematic, explicit or inferential. One example is the nature and implications of the distinction between a cultural anthropology modeled after the *Naturwissenschaften* (natural science based on the quantitative study of lawful relations between nomothetic data) versus one mirrored after the *Geisteswissenschaften* (cultural history based on the qualitative study of the constitutive relation between the interpretative mind and its mediated perception of idiographic symbols). The philosophical differences between, and the practical consequences of, the two alternative paradigms are quite explicit in the case of nineteenth-century German social science; they are relatively generalized, latent, problematic and inferential in the case

of the origin and development of American anthropology. The point is that they are operative in the latter instance as well. For example, Boas' anthropology reiterated but failed to integrate the distinction between *Natur* and *Geisteswissenschaft* (see Stocking, 1965b); Kroeber accepted an ontological 'super-organic' but he rejected an idiographic epistemology (see Bidney, 1953); Harris' recent *The rise of anthropological theory* (1968) explicitly utilizes the distinction-if only as a devisive and normative vehicle for the Olympian mock battles created by the author's imaginary historical voyages.

The paradigmatic distinction between the *Natur* and *Geisteswissenschaften* is of continued relevance, if inferentially so. Simonis, for example, brilliantly argues that a similar and unresolved distinction underlies the structural anthropology of Lévi-Strauss and that it crystalizes the ambiguous and problematic nature of structuralism's entire enterprise. To summarize (see Scholte, 1969b; see also Diamond, 1971; Scholte 1972b; Simonis, 1968), there is a permanent schism in structuralism that results from using a natural scientific and a cybernetically reductionist discourse to analyze (away) the intellectually constitutive and symbolically conscious domain of human culture. If human intelligibility moves from the natural to the cultural, scientific explanation returns the cultural to the natural. In thus reducing the substantive, existential, symbolic and cultural circumstance of concrete humanity to a relational, infrastructural, syntactic and neurological unconscious, Lévi-Strauss himself is forced to speak metaphorically (or remain silent) about a universal and hidden reality to which his own intentionally structured and culturally mediated discourse cannot be metonymically related. Simonis rightly concludes that 'structuralism cannot be a science; as it approximates its realization it can only be metaphor ...; it is transformed into a logic of the aesthetic, it is not a science of the human spirit' (Simonis, 1968, pp. 312, 348, my translation).

Systematic philosophical tendencies and 'quasi-metaphysical commitments' (Kuhn, p. 41) are present in most if not all anthropological paradigms, 'hardheaded' positivism included (see Kolakowski, 1968). Their very generality and often 'preconscious' foundations make them all the more important and pervasive. The mere intention of the scientific attitude to 'bracket' the metaphysical quest in no way precludes its operative, if latent, presence. Wolff gives a comparative example in his summary of the 'metaphysical tendencies of American and European sociology'. They differ along several dimensions: 'Scientific Truth'

compared to 'Existential Truth', 'Individual-Psychological Realism' contrasted with 'Social-Historical Realism' and 'Social-Historical Nominalism' versus 'Individual-Psychological Nominalism' (Wolff, 1959, p. 582). I have elsewhere (Scholte, 1966) compared the differences between Anglo-American empiricism and French-Continental rationalism in a similar vein: 'The adherents of the Anglo-American tradition, in its widest sense, assume the primacy of the behavioral act, their methods are essentially quantitative and descriptive, and their problems are phrased in diacronic-causal and empirically-inductive terms. The protagonists of the French anthropological tradition generally assume the primacy of the human mind, their investigations proceed along formal and structural lines, and their questions are posed in synchronic-relational and logically-deductive terms.' (See also Gross, 1968, p. 19ff, or the Pouwers–Köbben debate in *Bijdragen*.)

The general importance of these cursory examples is clear: 'Not being pure, that is, self-conscious, self-correcting, neither American nor European sociology [and anthropology] wholly reject [their] own metaphysical inclinations. As sciences, they should withold an "accent of reality," whereas, in fact, they bestow it, although each of them bestows it on a different sphere' (Wolff, 1959, p. 581). Just as importantly, philosophical 'prejudgments' are equally operative in the self-reflexive and critical activities of the sociologist of knowledge (see Wolff, 1959, pp. 587–590) and of the ethnologist of anthropological traditions (see Scholte, 1971b). Such 'circularity' is inherent in self-conscious reflection.

Paradigmatic domains are, as I have said, interrelated. Philosophical assumptions, too, are more than indigenous parts of syntagmatic discourses; they are also supportive of, and are in turn supported by, concommitant historical, sociocultural and existential contexts. One example will suffice. French intellectual history may in part be understood in terms of the mutable and competitive relation between Cartesian rationalism and the philosophy of spontaneity. The alternatives are philosophically definable. Cartesian rationalism 'is most perfectly exemplified in the scientific mentality. Seeking to create order from apparent chaos, the scientific mentality attempts to replace the richness of individual example with the precision of general law'. The Philosophy of Spontaneity, in contrast, 'glorifies artistic creation, the *espirit de finesse*, personal invention, romantic subjectivism, and a thirst for "life," captured in that mystical conception of *élan vitale*' (Clark, 1968, pp. 3–4). But

equally important are the socio-cultural correlates of the alternative philosophies. The philosophy of spontaneity is closely identified with proletarian political movements (anarchism, socialism and syndicalism) and aristocratic counter-movements (like romantic nationalism). Both share a distaste for bourgeois institutions. Cartesian rationalism, in contrast, 'may be identified with order, hierarchy, authority, and the bevy of bureaucratic institutions that exemplify the *esprit géometrique*... . The bourgeoisie is the social group most closely identified with this configuration of characteristics' (Clark, 1968, p. 2). The Cartesian pattern is concretely embodied in France's education system (especially the Ecole Polytechnique) and is most dramatically represented by some of its major social thinkers (e.g., Saint-Simon, Comte and Durkheim).

The intimate relations that exist between the paradigmatic domains again evidence the underlying and ever-present dialectic between social *praxis* and scientific activity. This dialectic is arrested in a syntagmatic framework. Since experience and reality, performance and competence, etc., are assumed to be discontinuous, there is no further contextualization, radicalization or critique of scientific premises, procedures or results. They therefore never become visible as circumstantial *ethno*logics of a more inclusive *Lebenswelt*. Ironically, the very self-sufficiency supposed by a syntagmatic framework is itself socially motivated and socially consequential. Its genesis and results are conservative, fetishistic, bureaucratic and uncritical.

What if the social sciences and their claims to objectivity presuppose a prereflexive totality (see Husserl, 1954, or Verhaar, 1970) and what if they are themselves social emergents (see Arendt, 1958, p. 271, or Merton, 1957, p. 456ff.)? How would this effect our understanding of anthropological *praxis*? What, in other words, is the diacritical function of a paradigmatic perspective on cultural anthropology's philosophical domain? The answers are to challenge the syntagmatic concept of fact and to propose a context-sensitive notion of facticity. Instead of generating still other reified discourses on the scientifically knowable universe, anthropological activity requires a more spohisticated understanding of constitutive universes of mediated discourse-its own and others. As I have said, anthropological paradigms are themselves symbolic forms. They create and posit specific realities; they are not isomorphic with reality. An anthropo*logic* is not a gift of God but a function of paradigmatic context.

The critical importance of a paradigmatic stance becomes especially

evident in considering the question of 'fact'. In the comparative study of ethnophilosophies, cultural anthropologists often and rightly insist that domains of facticity are contextual and relative. In their own *praxis*, however, they still tend to cling to an abstract notion of 'fact'. From a paradigmatic point of view, this is not only inconsistent but erroneous. If 'the availability of cultural "data" for the purposes of scientific understanding depends upon a possibility of participating in the processes and relationships of meaning productions' (Fabian, 1970, p. 5), then 'facts' are products, results and accomplishments of productions, inventions and activities. They are not epistemological givens.

In terms of the existential domain, 'facts obtain their specific "meaning" only by being facts for a specific person acting in a specific situation–or by being facts for the researcher himself' (Apel, 1967, p. 29). For the social domain, facticity is a function of 'socially organized common practices'. The selective perception and scientific analysis of facts presuppose a consensual agreement among member scientists. Such consensus entails 'ad hocing' and 'artful practices' as well as explicit rules of procedure and replicable criteria of verification (see Garfinkel, 1972, p. 313ff.). The historical mediation of domains of facticity is fairly obvious: 'not even the facts of science are facts for the unchanging "subject as such" ([or] of the "language as such") ... They are constituted in a concrete and therefore historically determined human horizon of meanings' (Apel, 1967, p. 33). Finally, as far as the philosophical domain itself is concerned, a paradigmatic stance demands a critical awareness of anthropological understanding as a contextual event, of specific facts as partial cases of 'imputed realities' (Gouldner in Boult, 1969, p. xxii) and of the realization that, in Goethe's words, 'every fact is already theory' (Diamond, 1971, p. 31).

Replacing the notion of fact with the idea of facticity has the other advantage of liberating cultural anthropology from the narrow confines of inductive empiricism. If scientific *praxis* is an intentionally motivated and creatively constitutive (if culturally mediated) activity, we can no longer assume that the relation between fact and theory, objectivity and verification, etc., is simply mechanical. The correlation between ethnological theory and ethnographic datum, for example, is not merely one in which theories are inductively derived from, and empirically verified by, the facts. Data are also selectively deduced from, and logically justified in term of, differing epistemic paradigms. As Mills points out, 'epistemologies have differed widely as to the manners in which empirical

elements enter into knowledge. But however variously they have incorporated empirical elements, in looking at the world for verification their concepts conditioned what they have seen' (Mills, 1963, p. 459). Similarly, Kuhn (1962, p. 125) remarks that 'far more clearly than the immediate experience from which they are in part derived, operations and measurements are paradigm-determined'. Fact-finding and empirical verification, in other words, are only one side of the scientific coin. Data are also a function of methods, methods of epistemology, epistemology of human insight and human insight, finally, of paradigmatic context.

The paradigmatic mediation of anthropological facts is an asset not a detriment. Without 'at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism' (Kuhn, 1962, p. 17), we would only gather facts at random and *ad infinitum* without ever knowing how to determine which empirical facts are pertinent to the issue at hand. That these paradigmatic criteria for selection, evaluation and criticism are themselves culturally mediated is clear. That is precisely why their contextual and comparative circumstances should be explicated. And that is also why, in the final analysis, the relation between anthropological *praxis* and anthropological knowledge is and should be a dialectical one.

3. Conclusion and prospects

If anthropological knowledge is culturally mediated, what are the critical implications for anthropological *praxis*? Are we simply faced with the well-known and apparently solipsistic *non sequitur* reductive relativism? (See Orans, 1970, p. 176.) Or are we instead to conclude that it is precisely the paradigmatic mediation of anthropological knowledge that makes any knowledge whatsoever possible and that a paradigmatic perspective must therefore command anthropological activity?

Before answering, one erroneous conclusion should be anticipated and corrected. The implications of a paradigmatic stance are relational and hence relativistic, but they are not therefore derogatory and reductionistic. Showing that anthropological paradigms are existentially, socially, historically and philosophically mediated is not identical to debunking one's colleagues or adversaries for their selfish motives, vested interests, historical falsification and ideological misrepresentation. To as-

sert that anthropological knowledge is contextually situated and comparatively relative is not synonymous with reducing anthropological *praxis* to egomania, contamination, myth or falsity. Paradigmatic mediation entails derivation not reduction; comparative differences imply partialities not perversions.

A paradigmatic framework is, however, relational and relativistic. If knowledge is contextually and comparatively mediated, 'connected with the realization that intelligibility takes many and varied forms is the realization that reality has no key' (Winch, 1958, p. 102). This conclusion is acceptable to most anthropologists, as long as it is applied to their 'object' of study and not their own *praxis*. The double standard is a curious one: 'while anthropology is justified in regarding the specific and varying [paradigmatic] standards of different cultures as relative to these cultures, its own scientific procedure involves the acceptance of standards which are universal and objective' (de Lagune, 1941, p. 261). The argument is justified on syntagmatic grounds. The double bind involved in the relativistic paradox would render anthropological *praxis* culture-bound and nonobjective: 'if cultural relativism is true in the objective sense, it must be applicable to itself as a cultural element. But in that case, it can, like all cultural modes of thought, be significant and valid only to the culture to which it belongs. On the other hand, if it is merely relative to our own culture, it is not universally applicable to all cultures, as the anthropologist assumes, and no objective science of anthropology is possible' (de Laguna, 1941, p. 239).

Though I do not mean to minimize the logical dilemma involved, a paradigmatic perspective must concur with the 'paradoxical' position that de Laguna and others seem to find so unbearable, the position that unconditional objectivity is impossible, and that anthropological *praxis* is valid primarily in relation to the culture to which it belongs. Objectivity is never total and static; it is always partial and dynamic (compare Diamond's, 1963. p. 3, paraphrase of Kierkegaard's conviction that 'no total or totally objective system is scientifically possible, because man in history is never complete'). Similarly, anthropological *praxis* is never entirely culture-free; it is in large part culturally motivated and culturally committed (this is true for certain variants of cultural relativism itself, those who mistakenly assert that cultural forces shape human beings instead of correctly interpreting cultural relativism to mean that cultures are created by men and women; see Diamond, 1971, p. 24; Gouldner, 1970, p. 440ff.; or Scholte, 1971a, p. 792).

The conclusion that anthropological knowledge is a conditional and mediated achievement is in no sense a disabling or negative one: the complementarity of systems, the possibility of different, yet independently consistent, purposive interpretations, faithful to the data, is in the very nature of historical [and anthropological] knowledge' (Diamond, 1964 a, p. 432). 'Perspective variations' (Husserl's term) in our paradigmatic positions are the ethnological continuations of the cultural realities ethnographers describe and ethnologists analyze. They are also the logical outcome of scientific *praxis* itself, of the principles of relativity, complementarity and uncertainty (see Matson, 1964, p. 113ff.; or Mannheim in Wolff, 1959, p. 571). Unmediated understanding is simply impossible. Understanding *is* mediation (Duerr, 1970, in *Current Anthropology*, 11 [1], p. 75). If anthropology is motivated by a desire to bring someone 'distant' (historically or geographically) 'closer' to us, then our paradigmatic circumstances are the irreducible *via media* that create the possibility of understanding our own and others' concrete humanity. Whether we call this discovering the 'I' in the 'Thou' (Buber) or the 'me' in the 'other' (Rousseau), the process as a whole is clearly a relational, conditional, constitutive and dialectical one. It 'represents the continuous expansion of knowledge, and has as its goal not achievement of a super-temporally valid conclusion but the broadest possible extension of our horizon of vision' (Mannheim, 1936, p. 109). If instead we reify and sever the relation between experience and reality, *praxis* and knowledge, we will never 'come to the point where the false ideal of a detached, impersonal point of view [can] be replaced by the ideal of an essentially human point of view which is within the limits of human perspective, constantly striving to enlarge itself' (Mannheim, 1936, p. 297).

This, then, answers our initial question: anthropological knowledge *is* mediated knowledge. A paradigmatic framework is therefore necessary because we must always learn to look upon ourselves as we look upon others. But this still leaves another problem unresolved: how do we choose between alternative paradigms? Is such a choice a mere question of tossing a coin? (see Orans, 1970, p. 176.) How do we 'steer clear between the Scylla of historic [and cultural] relativism ... and the Charybdis of ideologization through utopian historicism [or syntagmatic reification]?' (Radnitzsky, 1968, p. 35.) We need to develop a paradigmatic position which is not only contextual, comparative and perspectivistic but also one which is discontinuous, critical and evaluative. We have

to confront, in other words, the fact that 'the reflexive assimilation of a tradition is something else than the unreflected continuation of a tradition' (Ahlers, 1970, p. 116). But how?

I have outlined in this essay the first step – to describe and to analyze anthropological activities and traditions as mediating processes and mediated products. These descriptions and analyses should reveal the paradigmatic assumptions and 'ethnoscientific' limitations of differing anthropological positions. Given two or more alternatives, 'the first step is to ask which of them permits the understanding of the other as a social and human phenomena, reveals its infra-structure, and clarifies, by means of an immanent critical principle, its inconsistencies and limitations' (Goldmann, 1969, p. 52).

Whatever 'immanent critical principles' are chosen, they, too, will be paradigmatically mediated. As I have pointed out on several occasions, such 'circularity' inheres in a critical dialectic. Even the turn to a paradigmatic stance is intentionally motivated, socially conditioned, historically situated and philosophically inspired. As I have also said before, this is a definite asset not a mere detriment. If diacritical principles were not also 'located', they would be abstractly vague rather than concretely relevant. There is, in this sense, no escaping the 'hermeneutic circle' (e.g., Apel, 1967, p. 10 or Gadamer 1970, p. 80ff.).

Determining the specific critical principles to be chosen is, at this time, a point of conjecture (see Scholte, 1971a, 1971b). The syntagmatic and bureaucratic threats to our intellectual and social freedoms will undoubtedly be a crucial determinant in the choices we make. Certainly in this day and age, a liberated anthropology and an anthropology of liberation are complementary requirements. If the much-abused phrase 'the truth shall set us free' is to be taken seriously, it is also clear that, in the normative quest for an emancipated and emancipatory anthropology, a syntagmatic framework will never suffice; a paradigmatic stance will always be central.

References

Ahlers, Rolf (1970), Is technology intrinsically repressive?, *Continuum,* 8 (1): 111–112.

Albert, Ethel M., and Kluckhohn, Clyde (1959), *Values, ethics and aesthetics.* Glencoe, Ill., Free Press.

Apel, Karl-Otto (1967), *Analytic philosophy of language and the Geisteswissenschaften.* Dordrecht, Reidel.

Arendt, Hannah (1958), *The human condition.* Chicago, University of Chicago Press.

Aron, Raymond (1939), *German sociology.* New York, Free Press.

Bataille, George (1956), Un Livre humain, un grand livre, *Critique,* 10 (105): 99–112.

Bateson, Gregory (1972), *Steps to an ecology of mind.* New York, Ballantine.

Bennett, John W. (1946), The interpretation of Pueblo values: A question of values, *Southwestern Journal of Anthropology,* 2 (4): 361–374.

Berreman, Gerald D. (1962), *Behind many masks.* Ithaca, N. Y., Society for Applied Anthropology (Monograph Number 4).

— (1966), Anemic and emetic analyses in social anthropology, *American Anthropologist,* 68 (2): 346–354.

— (1971), Bringing it all back home. Paper presented to the 15th Annual Kroeber Anthropological Society Meeting, May.

Bidney, David (1953), *Theoretical anthropology.* New York, Columbia University Press.

Boult, Gunnar (1969), *The sociology of research.* Carbondale, Ill., Southern Illinois University Press.

Bourdieu, Pierre (1967), Systems of education and systems of thought, *International Social Science Journal,* 19 (3): 338–358.

Bowen, Elenore S. (1964), *Return to laughter.* Garden City, N. Y., Doubleday.

Bultmann, Rudolph (1955), *Essays philosophical and theological.* New York, Macmillan.

Burridge, K. O. L. (no date), Fieldwork, explanation, and experience. Manuscript.

Castaneda, Carlos (1968), *The teachings of Don Juan: A Yagui way of knowledge.* New York, Ballantine.

Clark, Terry N. (1968), Culture, social structure, and intellectual currents in nineteenth-century France. (Draft for Chapter 1 of *Institutionalization and innovations in higher education: Social research in France, 1815–1914*). Manuscript.

Darnell, Regna (1971), The professionalization of American anthropology: A case study in the sociology of knowledge, *Social Science Information,* 10 (2): 83–103.

De Laguna, Grace (1941), Cultural relativism and science. In *On existence and the human world.* New Haven, Yale (1966). Pp. 232–262.

Derrida, Jacques (1967), *L'Ecriture et la différence.* Paris, du Seuil.

Diamond, Stanley (1963), The search for the primitive. In Iago Goldstein (Ed.), *Man's image in medicine and anthropology.* New York, International University Press. Pp. 1–60.

— (1964a), A revolutionary discipline. *Current Anthropology*, 5 (5): 432–437.
— (1964b), Nigerian discovery: The politics of field work. In Arthur J. Vidich, Joseph Bensman and Maurice R. Stein (Eds.), *Reflections on community studies*. New York, John Wiley & Sons. Pp. 119–154.
— (1971), Anthropology in question. In Dell H. Hymes (Ed.), *Reinventing anthropology*. New York, Pantheon.
— (1974), *In search of the primitive: a critique of civilization*. New Brunswick, N. J., Transaction Books (distributed by E. P. Dutton).
Du Bois, Cora (1963), The curriculum in cultural anthropology. In David G. Mandelbaum, Gabriel W. Lasker and Ethel M. Albert (Eds.), *The teaching of anthropology*. American Anthropological Association Memoir 94. Pp. 27–38.
Fabian, Johannes (1970), Anthropological understanding through encounter: Magic or epistemology? Manuscript.
— (1971), Language, history, and a new anthropology, *Journal for the Philosophy of the Social Sciences*, 1 (1): 19–47.
Freilich, Morris, Ed., (1970), *Marginal natives: Anthropologists at work*. New York, Harper & Row.
Gadamer, Hans-Georg (1970), On the scope and function of hermeneutical reflection, *Continuum*, 8 (1): 77–95.
Garfinkel, Harold (1972), Remarks on ethnomethodology. In John J. Gumperz and Dell H. Hymes (Eds.), *Directions in sociolinguistics: The ethnography of communication*. New York, Holt, Rinehart & Winston. Pp. 301–324.
Geertz, Clifford (1967), The cerebral savage: On the works of Claude Lévi-Strauss, *Encounter*, 28 (4): 25–32.
Goldmann, Lucien (1969), *The human sciences and philosophy*. London, Cape.
Gonzalez, Nancie L. Solien (1970), Cakchiqueles and Caribs: The social context of fieldwork. In Moris Freilich (Ed.), *Marginal natives: Anthropologists at work*. New York, Harper & Row. Pp. 153–184.
Gough, Kathleen (1968), Anthropology and imperialism, *Monthly Review*, 19 (11): 12–27.
Gouldner, Alvin W. (1970), *The coming crisis in Western sociology*. New York, Basic Books.
Grace, George W. (1969), Notes on the philosophical background of current linguistic controversy, *Working Papers in Linguistics* (University of Hawaii), 1, January.
Gross, Llewellyn Z. (1968), Intellectual journey, *The American Behavioral Scientist*, 12 (1): 19–25.
Habermas, Jurgen (1971), *Knowledge and human interest*. Boston, Beacon Press.
Hallowell, Irving A. (1965), The history of anthropology as an anthropological problem, *Journal of the History of the Behavioral Sciences*, 1 (1): 24–38.

Halpern, Joel M., and Hammel Eugene A. (1969), Observations on the intellectual history of ethnology and other social sciences in Yugoslavia, *Comparative Studies in Society and History,* 11 (1): 17–26.

Harris, Marvin (1968), *The rise of anthropological theory: A history of theories of culture.* New York, Thomas Crowell.

Hellesnes, Jon (1970), Education and the concept of critique, *Continuum,* 8 (1): 40–51.

Helm, June, Ed., (1966), *Pioneers of American anthropology: The use of Biography.* Seattle, University of Washington Press.

Hodgen, Margaret T. (1964), *Early anthropology in the sixteenth- and seventeenth-centuries.* Philadelphia, University of Pennsylvania Press.

Husserl, Edmund (1954), *The crisis of European sciences and transcendental phenomenology.* Evanston, Ill., Northwestern University Press.

Hyman, Stanley E. (1962), *The tangled bank: Darwin, Marx, Frazer, and Freud as imaginative writers.* New York, Atheneum.

Hymes, Dell H. (no date), *Towards communicative competence.* Philadelphia, University of Pennsylvania Press.

Köbben, A. J. F. (1966), Structuralism versus comparative functionalism: Some comments, *Bijdragen tot de Taal-, Land- en Volkenkunde,* 122 (1): 145–150.

Kolakowski, Leszek (1968), *The alienation of reason: A history of positivist thought.* Garden City, N. Y., Doubleday–Anchor.

Krader, Lawrence (1966), Primary reification and primitive mythology, *Diogenes,* 56: 51–71.

— (1967), 'Persona' et culture: Réification et hypostatisation de l'homme dans la société civilisée, *Les Etudes philosophiques,* 3: 289–300.

— (1968), The interrelation of anthropological traditions as a dialectic. Manuscript.

— (1968b), Person and collectivity: A problem in the dialectic of anthropology, *Transactions of the New York Academy of Sciences,* Series II, 30 (6): 856–862.

Laing, R. D., and Cooper, D. G. (1964), *Reason and violence: A decade in Sartre's philosophy, 1950–1960.* London, Tavistock.

Lefebvre, Henri (1966), *Le langage et la société.* Paris, Gallimard.

Lepenies, Wolf (1971), *Soziologische Anthropologie: Materialen.* München, Hanser Verlag.

Lévi-Strauss, Claude (1946), La Technique du bonheur, *Esprit,* 127: 643–652.

— (1955), *Tristes tropiques.* New York, Atheneum.

— (1960), *The scope of anthropology.* London, Cape.

— (1963), Rousseau: The father of anthropology, *The UNESCO Courier,* 16 (3): 10–14.

Lips, Julius E. (1966), *The savage hits back.* New Hyde Park, N. Y., University Books.

Malinowski, Bronisław (1967), *A diary in the strict sense of the term.* New York, Harcourt, Brace & World.

Manners, Robert O., and Kaplan, David (1968), *Theory in anthropology.* Chicago, Aldine.

Mannheim, Karl (1936), *Ideology and utopia.* New York, Harvest Books.

Maquet, Jacques J. (1962), Some anthropological contributions to the sociology of knowledge, *Social Science Information,* 1 (3): 5–20.

Matson, Flloyd W. (1964), *The broken image: Man, science and society.* Garden City, N. Y., Anchor Books.

Merleau-Ponty, Maurice (1951), The philosopher and sociology. In Maurice Natanson (Ed.), *Philosophy of the social sciences.* New York, Random House. Pp. 487–505.

Merton, Robert K. (1957), *Social theory and social structure.* New York, Free Press.

Mills, C. Wright (1959), *The sociological imagination.* London, Oxford.

— (1963), *Power, politics, and people: The collected essays of C. Wright Mills.* New York, Ballantine.

Monod, Jean (1970), Un Riche cannibale: Ethno-récit, *Les Temps Modernes,* 293–294; 1061–1120.

Moscovici, Serge (1966), L'Histoire des sciences et la science des historiens, *Archives Européennes de Sociologie,* 8: 116–126.

Murphy, Robert F. (1971), *The dialectics of social life: Alarms and excursions in anthropological theory.* New York, Basic Books.

Myrdal, Gunnar (1969), *Objectivity in social research.* New York, Pantheon.

Nutini, Hugo (1968), A comparison of Lévi-Strauss' structuralism and Chomsky's transformational generative grammar. Manuscript.

Orans, Martin (1970), Social organization. In Bernard J. Siegel (Ed.), *Biennial review of anthropology.* Standard, Calif., Stanford University Press. Pp. 132–190.

Palson, Chuck (1970), The desirability of various types of graduate departments, *AAA Newsletter,* 11 (8): 6–7.

Pouillon, Jean (1968), Tradition: Transmission ou réconstruction? Manuscript.

Pouwers, J. (1966a), The structural and functional approach in cultural anthropology, *Bijdragen tot de Taal-, Land- en Volkenkunde,* 122 (1): 129–144.

— (1966b), Referential and inferential reality: A rejoinder, *Bijdragen tot de Taal-, Land-, en Volkenkunde,* 122 (1): 151–157.

Radnitzsky, Gerard (1968), *Continental schools of metascience.* Göteborg, Akademiförlaget.

Schmid, Herman (1968), Science and the control of social science, *The American Behavioral Scientist,* 12 (4): 59–64.

Scholte, Bob (1966), Epistemic paradigms: Some problems in cross-cultural research on anthropological theory and history, *American Anthropologist,* 68 (5): 1192–1201.

— (1969a), Lévi-Strauss' Penelopean effort: The analysis of myths, *Semiotica,* 1 (1): 99–124.

— (1969b), Review of Yvan Simonis' *Claude Lévi-Strauss ou 'la passion de l'inceste': Introduction au structuralisme, American Anthropologist,* 71 (3): 504–505.

— (1970), Toward a self-reflexive anthropology: An introduction with some examples, *Critical Anthropology,* 1 (2): 3–33.

— (1971a), Discontents in anthropology, *Social Research,* 38 (4): 777–807.

— (1971b), Toward a critical anthropology. In Dell H. Hymes (Ed.), *Reinventing anthropology.* New York, Pantheon.

— (1972a), The structural anthropology of Claude Lévi-Strauss. In John J. Honigmann (Ed.), *Handbook of social and cultural anthropology.* Chicago, Ill., Rand McNalley.

— (1972b), Lévi-Strauss and La Pensée Leachiènne (With Yvan Simonis), *Semiotica* 6 (3): 289—294.

Simonis, Yvan (1968), *Claude Lévi-Strauss ou 'la passion de l'inceste': Introduction au structuralisme.* Paris, Aubier-Montagne.

Stocking, George W., jr. (1965a), On the limits of 'presentism' and 'historicism' in the historiography of the social sciences, *Journal of the History of the Behavioral Sciences,* 1 (3): 211–218.

— (1965b), From physics to ethnology: Franz Boas' artic expedition as a problem in the historiography of the behavioral sciences, *Journal of the History of the Behavioral Sciences,* 1 (1): 53–66.

Urbanowicz, Charles F. (1970), Mother nature, father culture. Paper delivered at the 8th Annual Meeting of the Oregon Academy of Sciences, February.

Verhaar, John (1970), Method, theory, and phenomenology. In Paul L. Garvin (Ed.), *Method and theory in linguistics.* The Hague, Mouton. Pp. 42–91.

White, Leslie (1966), *The social organization of ethnological thought.* Houston, Texas, Rice University Monographs in Cultural Anthropology, 25 (4).

Winch, Peter (1958), *The idea of a social science and its relation to philosophy.* New York, Humanities Press.

Wolf, Eric R. (1969), American anthropologists and American society. In Stephen A. Tyler (Ed.), *Concepts and assumptions in contemporary anthropology.* Athens, Georgia, University Press. Pp. 3–11.

Wolff, Kurt H. (1959), The sociology of knowledge and sociological theory. In Llewellyn Gross (Ed.), *Symposium on sociological theory.* Evanston, Ill., Row, Peterson. Pp. 567–602.

— (1964), Surrender and community study: The study of Loma. In Arthur J. Vidich, Joseph Bensman and Maurice R. Stein (Eds.), *Reflections on community studies*. New York, John Wiley. Pp. 233–263.
— (1971), This is the time for radical anthropology. In Dell H. Hymes (Ed.), *Reinventing anthropology*. New York, Pantheon.

4 Anthropological traditions: Comparative aspects

ÅKE HULTKRANTZ

The formation of separate traditions is a natural matter in all branches of humanistics. In some disciplines traditions grow so far apart from each other that they may appear almost incompatible. Ethnology is one of these disciplines. My first impression of ethnology many years ago was that not only the subject matter, but also the scientific opinion on the subject matter, was kaleidoscopic. As a young student of Gerhard Linblom's at the University of Stockholm, I had to learn in detail not only the old, evolutionary schools but also the widely differing historical schools created by Boas, Wissler, Graebner, Schmidt, Elliot-Smith, Perry, etc. Having studied other humanistic disciplines, it seemed to me that ethnology more than any other subject was made up of bewildering scientific interpretations. And I felt, certainly not altogether unjustly, that this chaotic situation reflected the dubiety in perspectives and methods in this discipline. Malinowski's and Radcliffe-Brown's rejection of the 'conjectural methods' of historical ethnology was in a way a natural outgrowth of this situation.[1] However, my first impression of chaos in ethnology was succeeded by a second impression of 'order in chaos', for the many schools were found to be functions of more uniform scientific traditions.

The significance of diverging national and continental traditions in ethnology was of less practical consequence before World War II. In each country in Europe and North America where the ethnological discipline existed in its own right or as a subdiscipline of anthropology,

1. It is another matter that ahistorical structuralists such as Durkheim, Radcliffe-Brown and Levi-Strauss have in turn rejected the schools of their predecessors and have introduced their own.

different schools followed their respective programs, using the materials produced by other schools but applying their own methods to them. After the war this situation changed; mutual understanding between the ethnological doctrines of different countries became a necessity in a world which was thought of as becoming unitary and closely integrated. From a certain angle the theoretical split in ethnology appeared obsolete and destructive. When slogans of unification (such as Wilkie's One World) guided public aspirations; the dissension of ethnology and anthropology was experienced by many devoted scholars as anomalous. Man is one, had long been the claim of American anthropologists, and in the post-war athmosphere this was felt as intensely as ever before; one European ethnologist even wrote a book with that title (Gjessing, 1948). Man was one, but the science of man was certainly divided, and it has remained that way. As years have passed, many have found it increasingly uncomfortable to watch the diversification of ethnology and cultural anthropology along traditional lines which are more or less national. There are, of course, several instances of the negative effects of this partition. For instance, too often ethnological congresses are divided into sections which appeal only to students of a particular nationality (Hultkrantz, 1965b, p. 91). Differences in scientific attitudes tend to follow national lines (*cf.* Murdock, 1951; Firth, 1951). As I have tried to show, the laudable expansion of anthropology promoted by the journal *Current Anthropology* is largely a one-sided expansion of the American idea of anthropology (Hultkrantz, 1965a).

At the same time, however, the dissension in ethnological theory has had a positive value in other respects. Traditions fostered in specific national milieus may fertilize each other and create new and fresh methodological devices. We may here think of the fusion of Boas' and Durkheim's methods in the United States, or of Anglo-Saxon functionalism and French structuralism in France. In smaller countries, such as the Scandinavian, influences from the great traditions of other countries have coalesced and formed new, although not very characteristic trends. The more eccentric schools have found little response here. Without doubt, the cross-fertilizing influences have gained strength as a result of the new world situation.

Recently, the attention of anthropologists and ethnologists has been focused on the difference between American and European anthropology (Ishida, 1965). It is true that anthropology is a much wider field than ethnology, but the latter field was naturally touched upon insofar as

it is or was, for a long time, the central discipline within anthropology. In Europe, on the other hand, ethnology has often had the tendency to include other anthropological fields as well, at least in a certain measure. The difference between American anthropology and European ethnology is on the whole considerable (Hultkrantz, 1952), but the comparison between them is not meaningless. In particular, the ethnological key discipline, ethnology in a qualified sense, offers similarities on both sides of the Atlantic.

In the remainder of this essay it is my intention to outline some of the major traditions of ethnology in existence today and to discuss their prerequisites and possible prospects. First, however, we must pin down definitions of ethnology and ethnological traditions and pay attention to use of the word ethnology in a historical perspective.

Europeans usually think of ethnology as a discipline concerned with the comparative study of culture (*cf.* Hultkrantz, 1960, pp. 113–114). By 'culture' they mean, first, primitive and folk cultures; the so-called high cultures or civilizations are included in a varying degree, sometimes entirely, sometimes only for comparative reasons. The comparative study has its roots predominantly in historical perspectives. Some American anthropologists seem to embrace a similar definition of ethnology, making it synonymous with cultural anthropology. For instance, Lowie's textbook on cultural anthropology may well be called a text on ethnology, and his history of ethnological thought could as well be presented as a history of thought on cultural anthropology (Lowie, 1934, 1937). Herskovits (1959, p. 391) vaguely defined ethnology as 'the study of custom', 'the study of the nonbiological aspects of human behavior', that is, cultural anthropology. Jacobs and Stern (1949, p. 3) and Winick (1956, p. 193) determine ethnology in the same broad way. Nevertheless, Americans tend to speak of cultural or social anthropology when they analyze the structure and function of cultural systems, whereas they use ethnology mostly as the name of historical culture studies of primitive groups. The dominant historical aspect of ethnology is underlined by almost all European ethnologists, irrespective of which ethnological traditions they represent. It therefore seems appropriate to me to characterize ethnology, in this connection, as the study of culture from descriptive and historical points of view. Such a definition meets the demands in both America and Europe and probably in other areas as well. There is less agreement whether ethnology deals only with the so-called primitive peoples, or if it also includes the study of the 'folk'

of high-culture areas, perhaps even the total population in the higher civilizations.

The term ethnology comprises two Greek words, *ethnos* (meaning 'folk', 'people') and *logia* (meaning 'doctrine') and may be translated as 'the doctrine of peoples'. It has long been supposed that its promulgator was the Englishman W. F. M. Edwards, who in 1839 founded the 'Societe ethnologique de Paris'. Actually, the true inventor of the term was Chavannes (1787), and it was revived by the famous French physicist A. M. Ampere; as far as we know, he used it for the first time in 1830 (Rohan–Csermak, 1967).[2] Whereas Edwards concentrated his attention on the racial aspects of mankind, Ampère stressed the ideological, cultural and social as well as the racial aspects. In this comprehensive sense, ethnology was accepted as a discipline in Europe until the beginning of the twentieth century, sometimes alternating with anthropology.[3] In the United States anthropology entered as the name of the complex discipline, with ethnology as its subdiscipline, due to the influences of Joseph Henry and D. G. Brinton (Hallowell, 1960, p. 31; Brinton, 1892).[4] The main emphasis in both ethnology and anthropology was, at this time, on evolutionary and developmental research, a fact which is reflected in the concentration on primitive culture in contemporary works. This, in turn, explains why so many ethnologists still consider primitive culture their primary domain.

We are not concerned here with the ramifications of anthropology and ethnology in different countries or with all the schools formed from the common roots. Many of them have been important but are now virtually extinguished, such as the schools founded by W. Schmidt and G. Elliot Smith.[5] Instead, we shall review the main schools and traditions of ethnology today, concentrating our attention on the traditions rather than on the schools.

A clear distinction between 'tradition' and 'school' is thus necessary in this connection. In my view, a 'tradition' refers to a set of scientific

2. I suppose that Rohan-Csermak's continued research will show to what extent the term ethnology was coined on the pattern set by the term ethnography which, as he notes, was used in Germany in the 1790s.

3. It is interesting to note that up to the present time ethnology has meant the study of races in India (Tax *et al.*, 1953, p. 222).

4. It is a strange coincidence that the promulgator of anthropology, Joseph Henry, was a physicist, as was Ampere.

5. Here I disregard the continuation of the Vienna school in some quarters in Italy.

attitudes and presumptions which have developed as a result of a gradual historical process. Such a 'tradition' is deeply rooted in the spiritual climate of a certain area, a nation or even a continent. As Hoebel says 'each type of anthropology is the product of a particular culture, developed to function in a special way in a special total environment' (Hoebel, 1958, p. 633). A 'school', on the other hand, comprises a group of scholars who deliberately pursue certain scientific goals by certain defined methods. The school presupposes a background tradition, although it may show affinities to other traditions as well. We may talk of the culture-historical tradition of continental European ethnology and, within this tradition, of the Vienna school (or formerly the *Kulturkreise*-school, called by its adherents the culture-historical school).

As far as I can see, we can reckon with four great ethnological traditions in the occidental world, separated from each other by different historical developments, and each still carrying a stamp of its own.

1. American ethnology

American ethnology is represented in the United States and in parts of Canada. The origins of this tradition have been given by Hallowell (1960, pp. 34–158); its development until recent times has been sketched by myself (1967b).[6] Since the 1880s American ethnology has been part of American anthropology, a circumstance which has possibly impeded the progress of historical thought in ethnology and has made it possible to contemplate discarding ethnology entirely. On the other hand, American ethnology must also be judged positively in its interplay with other anthropological subdisciplines such as physical anthropology, archaeology and linguistics. The interrelations among these subjects have widened the range of vision of the American ethnologist and fertilized his approaches to important marginal areas of the ethnological field.

It is difficult to characterize the American tradition of historical ethnology today since it involves so many different trends. One aspect, however, is most apparent: The extremely cautious attitude toward historical research on primitive cultures, a caution bordering on timidity.

6. *Cf.* also my extensive survey of the history of American ethnological studies in the religions of the North American Indians (Hultkrantz, 1966–1967).

This attitude has many causes (Hultkrantz, 1967b), of which one is the half-hearted approach to history evinced by the founder of the modern American anthropology, Franz Boas, and perpetuated by later generations. It is a moot question whether Boas was fundamentally historical or functional in his methods. We know for certain, however, that he aimed at explanations of culture in terms of function and process and that he shifted to functionalistic interpretations about 1910.

Another cause, admitted for instance by Kroeber and Herskovits, is the ahistorical tendency among American anthropologists, nourished by the lack of old traditions in America. It is true that a more direct interest in history was represented by some European-trained scholars of 'the great generation', such as Lowie, Radin and Wallis, but most of their colleagues became increasingly absorbed in practical issues outside the scope of historical ethnology. They welcomed the new trends in the 1920s whereby the culture concept was atomized, and research was concentrated in the direction of personality, acculturation and, later on, social structure.

A third cause is the onesided, character, mainly diffusionist, of the American ethnological approach until the 1930s. Although diffusion studies are still conducted, and rightly so, the old diffusionism fell into disrepute because of its sterile, mechanical culture-area concept and the 'flat perspective' (Kroeber) which was applied, largely because of the lack (at that time) of available documentary source materials in the study of North American Indians, the chief object of research until World War II.

It is an interesting fact that the gradual abandonment of historical procedures was correlated with a diminishing interest in studies of material culture. Before 1930 elements of material culture were the main tools of the diffusionistic approach. (Here I make an exception for Boas who, for his studies of geographic distribution, used myths and tales.) Today these kind of studies are very rare in the United States, despite recent admonitions to anthropologists to conduct investigations of museum pieces.

Historical ethnology in America has proceeded cautiously along the following lines. There is a continuing, but fading, research with refined diffusionistic methods, whose chief exponent is Driver. There is the combination of structural-functional and time perspectives, as represented by Eggan. There is the ecological approach to historical reconstruction, headed by Steward and characterized by an evolutionary

view.[7] Acculturation studies may also be termed historical, even if their main concern is the process rather than the sequence of historical facts. The main stream of present-day historical research is, however, to be found in ethnohistory. Ethnohistory, the investigation of historical time-depth of primitive cultures by the aid of written source documents, is vigorously carried out by some leading American scholars, e.g., Fenton, Ewers and Spicer. The sum total, therefore, is that within the American ethnological tradition, characterized by a hesitant approach to historical research, there are several schools, the foremost of which is ethnohistory.

2. European general ethnology

European general ethnology is represented in particular by continental European *Volkerkunde* and Scandinavian *etnografi*. In Great Britain and France ethnology plays a rather insignificant role, social anthropology being the dominant anthropological discipline. Where ethnology exists–only at a few museums in Great Britain with a teaching staff, e.g., the Pitt Rivers Museum in Oxford–it means historical ethnology. In France there is, in addition to ethnology, ethnography *(ethnographie)*, a discipline dedicated to descriptive and systematic studies and founded by Mauss.[8] However, the emphasis on historical ethnologies occurs primarily in Germany, Austria and Switzerland, and in Denmark, Norway and Sweden. Here the concept of ethnology is perhaps wider than n the Anglo-Saxon countries and France, comparative viewpoints other than historical being quite common; but the main stream is historical iand phenomenological (ethnographical).

A survey of the continental subtradition *(Volkerkunde)* has recently been written by one of its chief exponents (Heine-Geldern, 1964). He deals with its history from Waitz and Bastian, the latter the founder of German *Volkerkunde*, and concentrates on the historical perspective introduced by Ratzel and his pupil Frobenius in the late 1800s. Heine-Geldern summarizes 'the common denominators' of the ethnology of

7. Today evolutionism is fashionable again in American anthropology. Its most fundamental exponent is Leslie White. In a way, evolutionism represents a historical perspective, but from another angle it may be considered antihistorical.

8. Ethnographic approaches are also talked about in the United States and in England, but there is no so-called discipline or subdiscipline in these countries.

the German-speaking countries by stating that this ethnology comprises the study of all aspects of human culture and that it is a branch of the humanities and is concerned with historical research. He admits, however, that in all of these points Muhlmann dissents and goes his own way. Heine-Geldern (1964, p. 416) finds it difficult to delineate his exact position in ethnological theory. A hint of Muhlmann's general direction may, however, be gathered from his definition (1948, p. 17) of ethnology, as 'the social connection of the ethnical life communions' *(Lebensgemeinschaften)*, the stress being laid on the interactions of ethnical groups.

To return to Heine-Geldern, he shows that the Middle European ethnology in general has considered the primitive cultures its legitimate realm but has lately also included the old high civilizations, exactly as has happened in American anthropology. He concludes (1964, p. 416) by saying that 'the historical character of ethnology in the countries concerned has proved a sound basis which has spared ethnology those vagaries and ephemeral fashions which have occasionally plagued it elsewhere'. The situation is thus much different from that in North America.

If we disregard Muhlmann and his pupils, it is possible to differentiate between several minor activities and two main schools in the German-speaking area today. First, there is the modern Vienna school (if we really can call it a single school), characterized by a negative attitude to the old *Kulturkreise* schema and a sober insistence on the use of archaeological materials and methods for culture-historical reconstructions (Haekel, 1956). This new approach seems to represent, in a way, a fusion between the former adherents of the old Vienna school (Koppers, now deceased, and Haekel) and their former adversaries, the 'independent' Viennese ethnologists (Heine-Geldern and Jettmar). Second, there is the culture-morphological school *(Kulturmorphologie)*, founded by Frobenius and carried on by his students, e.g., Jensen, Schmitz and Haberland. This school has emphasized the spiritual aspects of culture, particularly myth and cult, and the creative power of spiritual experiences (Jensen, 1954; Freudenfeld, 1960).

The situation in the Scandinavian countries is both similar and different; I refer in this connection to an article on Swedish anthropology published some years ago in the *American Anthropologist* (Izikowitz *et al.*, 1959). Here, the links with geography were pointed out, and the fact was stressed that most research is done at the museums. As a result,

until recently much work has been devoted to archaeology. During the last two decades there has been an increasing interest in society and religion, the latter research conducted within both the confines of ethnology and a closely related anthropological discipline, comparative religion. The development in Denmark and Norway has been largely the same. The methods applied have been mostly historical, predominantly so in the study of material culture.

As I have said before, it is difficult to find divergent school formations within Scandinavian historical ethnology. If we disregard the archaeologists, who have methods of their own, we may discern two lines of research, the historical reconstructions-by-diffusion studies introduced by Birket-Smith in Denmark and the single-trait distribution studies inaugurated by Nordenskiold and Lindblom in Sweden. The latter and their followers have been called 'the Swedish school' (Herskovits, 1949, p. 175). The present ethnological activity in Scandinavia is characterized by a fusion of structuralistic, ecological and historical points of view.

3. European regional ethnology

European regional ethnology, or the study of European folk cultures, is sometimes called *Volkskunde* (German-speaking countries), laography (Greece) or folklife research (Scandinavia, Great Britain).[9] This tradition, is represented in all European countries west of Russia[10] and in some of them, e.g., Sweden, formerly held a stronger position than did general ethnology. It has different origins in different countries. Historical, antiquarian, national and folkloristic interests contributed to the formation of the discipline, and its relations to folklore have in many quarters, for instance, Germany, been indecisive. The links with history are firm (*cf.* Rank, 1962–1963), and the historical foundations of regional ethnology have been stressed repeatedly by its representatives (see the articles on the theme 'History and ethnology' in *Ethnologia Europaea*, 1967, I [2].) Although the key concept in general ethnology is culture, regional ethnologists have also launched such central concepts as 'folklife' (Erixon, 1951) and *ethnie* (Rohan-Csermak,

9. The concept of regional ethnology was created in 1937 by the recently deceased grand old man of this discipline, the Swede Sigurd Erixon (1937–1938).

10. Ishida has pointed out that 'folk ethnology' is a vital discipline in Japan as well, here labeled 'folklore' (Tax *et al.*, 1953, p. 223).

1965). There are, of course, many other divergent national subtraditions (for a sample, see Erixon, 1962; Heilfurth, 1961; Fenton, 1967).

Thus, regional ethnologists have focused their interest on the 'folk', the peasantry within the higher civilizations of Europe. The relation of the folk to the other groups of European society has been thoroughly investigated and stimulated much discussion about the mechanisms of cultural diffusion. The earlier preoccupation with traditional customs and folkways has been succeeded by a more conscious study of folk activities as an integrated part of culture. The historical outlook has continued to dominate, however, since the peasantry constituted a group with large historical depth and more and more tends to be a thing of the past. More recent interest in urban culture *(Grossstadtvolkskunde)* and increasing preoccupation with social structure may turn the tide away from history, at least partially. The growing feelings of mutual interests and common goals among regional ethnologists led them to create an organization of their own with a central journal, in the 1950s, *Laos*, in the 1960s, *Ethnologia Europaea* (issued in Paris).

4. Soviet ethnology or ethnography

Soviet ethnology or ethnography is 'a historical (or social) science studying the lifeways and cultures of the peoples' (Tokarev, 1967a, p. 31). Here we have to consider the double origins, the prerevolutionary Russian ethnology and the Marxist theories. The former started with such authors as Krasheninnikov and Weniaminov and reached its peak, perhaps, in the writings of Anutshin. Sternberg was the connecting link between tsarist ethnology and Soviet ethnology. In the post-revolutionary era, such researchers as Rudenko, Popov, Zelenin, Tokarev, Zolotarev and Okladnikov have made Soviet ethnology well known. Characteristic of Soviet ethnology is its Marxist foundation and its insistence on the formulation of evolutionary processes; it studies all cultures and societies, primitive and folk cultures as well as cultures and part-cultures pertaining to industrial societies. Tokarev points out that, in the latter cases, the objects of ethnological research may be shared with such disciplines as economics, law and art history, but the aspects considered are different. He delineates as the most common tasks of Soviet ethnology ethnogenesis, ethnogeography, the study of ethnic traditions and their expressions in the ways of peoples, the in-

vestigations of laws of cultural origins and of cultural diffusion, valuation of cultures and research on modern ethnic developments and transformations (Tokarev, 1967a). In another paper he outlines four directions of historical research in Soviet ethnology: Efforts to reconstruct early forms of society and culture, research on the ethnic history of different nations (ethnogenesis); concrete historical-ethnological monographs of various peoples; and historical aspects on present cultural forms (Tokarev, 1967b).

Tokarev writes that to a great extent the boundary lines between ethnological and historical research do not exist in the Soviet discipline. Tolstov even defines ethnology (ethnography) as 'the branch of history dealing with the cultures, origins, and distributions of individual peoples, and with their cultural-historical contacts' (quoted in Vucinich, 1960, p. 867). This emphasis of Soviet ethnologists on historical perspectives has also been observed outside of the Soviet area.[11] Jettmar (1964, p. 268), for example, notes that 'until recently no ethnological school saw the task of ethnology within the frame of history as much as the Soviet ethnological school'. Perhaps it is not quite right to talk about a school, if by school we mean one group of scholars standing apart from other groups. In this sense there are no real schools in the Soviet discipline; the ideological preconditions, the perspectives and the methods applied seem to be the same everywhere. On the other hand, the term tradition does not appear to be quite adequate either, for Soviet ethnology is a product not of a gradual development but of a deliberate attempt to guide a scientific course. In actual fact, the tradition takes on the appearance of a school, a deliberate program.

5. Future prospects

The impression we receive from this short survey of the four great traditions in present ethnology is that this discipline, as arranged around a historical core, could face a prosperous future, at least in the Old World. But this conclusion is scarcely borne out by all of the modern trends. Some anthropologists think that ethnological traditions are in a process of transformation or even extinction. They either refer to the

11. For a Western review of Soviet ethnology during the 1940s, see Shimkin, 1949.

general academic situation in our time, the reduction of historical tasks and the promotion of scientific tasks, or they watch the new developments in ethnology which show a changing pattern.

The most radical dismissal of historical ethnology took place in Great Britain in the 1930s when social anthropology as a matter of fact disowned ethnology. A similar development may take place in the United States where historical studies are not very fashionable. It all seems to have started here with Kroeber's (1953, p. 366) famous statement at the international symposium on anthropology in 1952: 'Now, how about ethnology? I am about ready to abandon this baby to the wolves–to a premature fate or to a senescent death as one may see it'. Kroeber justified his stand by referring to the fading of the old, primitive cultures and the speculative character of much of traditional ethnology; as we know, the latter point had been earlier stressed in Great Britain. We may add that Kroeber expressed what many of his American colleagues had probably thought for some time. The reasons he adduced seem strong in themselves but have deeper causes–the ahistorical, or even antihistorical, attitudes in America (discussed above) and the public demand of anthropologists who through analyses of more complex and modern societies can contribute to the solution of practical problems. It was this presumed usefulness of anthropologists which led foundations and governments to back anthropology.

Kroeber's statement is perhaps more shocking to European than to American ears. Since ethnology in the United States constitutes only a subdiscipline of anthropology, it may be dispensed with without detrimental effects on the position of anthropology or anthropologists. In Europe a similar change would require a complete reorientation of perspectives and methods in ethnology.

There is no doubt that ethnology has been kept in the background in modern American anthropology. It figures less often in the *American Anthropologist*, and even the venerable *Bureau of American Ethnology* has been reorganized to an office of anthropology in order to suit the times. Those daring students who still cling to ethnology make use of methods which do not allow too much speculation. And I am not certain that, in the long run, they will not restore ethnology to a solid position.

European general ethnology is also affected by the new course. Ethnological research was early replaced by social anthropological research in Great Britain, and in Finland Westermarck founded a school based

on the British pattern. Later British influence was a factor in stimulating Lévi-Strauss to reintroduce studies of social structure in France. The combined radiation from Britain and France remodeled ethnological research in a social-anthropological direction in Holland and Norway, and the same process is now under way in Sweden. The dilemma is that, unlike America, the European countries have to make a choice between ethnology and social anthropology, chairs that were formerly strictly ethnological now being reserved for social anthropologists. The obvious solution would be to have both subjects represented, but university finances do not generally allow this. Social anthropology is a modern, fashionable science; ethnology is not, and to some it has an antiquarian flavor–the hobby of the privileged minority in a class-structured society. This is, of course, a regrettable confusion of the amateur explorer and the academically trained field researcher and theorist!

The old historical tradition in European general ethnology has its stronghold on the continent where *Volkerkunde* allows the amalgamation of historical and culture-analytical points of view. Whether at last a similar struggle between history and science will emerge here is difficult to tell.

In European regional ethnology the historical outlook is in increasing measure being supplemented with synchronic perspectives. On the other hand, functionalism has long been integrated with Swedish folklife research without jeopardizing its general historical perspective. The new studies in urban culture and modern social groups may turn interest away from the historical investigations of rural and national traditions, at least in Scandinavia, but as yet we do not know enough to predict the future (*cf.* Erixon, 1967a, 1967b).

The same judgment might apply to Soviet ethnological research. The dynamics for a study of synchronic problems are contained in the Soviet ethnological system, despite its ultimate historical emphasis. The boundary lines between ethnology and sociology are indeed not always clear here (Tokarev, 1967a, p. 36).

Despite the evidence of a weakening of the historical dimension of ethnology after 1930, which was accentuated after World War II, other signs point to a positive reappraisal of historical studies. The most surprising event in this connection is the reorientation of British social anthropology. In 1961 Leach (p. 1) stated that the particular aim of this social anthropology, the comparative analysis of social structures,

seemed to have worked itself out. 'Most of my colleagues are giving up the attempt to make comparative generalizations; instead they have begun to write impeccably detailed historical ethnographies of particular peoples.' The development that led to this situation began with Evans-Pritchards's programmatical article on social anthropology in 1950, in which he said (p. 124), among other things, 'I expect that in the future there will be a turning towards humanistic disciplines, especially towards history, and particularly towards social history or the history of institutions, of cultures and of ideas'. Shortly afterwards Firth (1951, p. 488) declared that British social anthropology was beginning to change into 'a more systematic study of variation, including variation over time'.[12] From recent meetings with the British social anthropologists, it appears that this historical outlook is gaining ground, without supplanting traditional research in the discipline–as a matter of fact, there is also the contrary tendency to settle with the sociologists (Banton, 1964).

This example from Great Britain may be criticized on the grounds that here we are not dealing with ethnology but with social anthropology. However, designations of disciplines mean little in this context (*cf.* Tax *et al.*, 1953, p. 225). The main issue is that in a cultural anthropological discipline long marked by its nomothetical approach, there is a resumption of historical research, a renewed interest in historical perspectives. No doubt this will affect students of professional ethnology.

In looking forward it is of major importance that we stress the necessity of undertaking historical research, irrespective of what we want to call this research. In the long run, perhaps culture history will prove to be a better word, as was suggested at the anthropological conference in New York, 1952 (Tax *et al.*, 1953, pp. 218–220). European ethnologists have repeatedly emphasized that ethnology is nothing more than culture history. At the symposium on 'Anthropological horizons' (1960), at Wartenstein, Austria, a distinction was nevertheless made between ethnology and culture history, probably at the instigation of the chairman, A. L. Kroeber. Kroeber, who is certainly one of the seminal figures in American anthropology, stated (1962, p. 85) 'that ethnology has a rather limited future but that there will be for a long time to come some descriptive work to be done on primitive non-literates, which will be a diminishingly important function of the anthropologist'. This is believed

12. Against this background, it is surprising that Murdock in 1952 could criticize his British colleagues because they could not see 'social structure ... as a system moving and ordering through time' (Tax *et al.*, 1953, p. 224).

to be an ethnocentric interpretation of ethnology; research on the European folk, Asiatic high cultures and industrial society by West European and Soviet ethnologists as well as European regional ethnologists is not taken into account. It is apparent that Kroeber's definition of anthropology and ethnology was based or the American pattern (*cf.* Hultkrantz, 1965a). He did not acknowledge the plurality of ethnological traditions.

Yet, Kroeber (1962, p. 89) conceived culture history as 'total' history, that is, 'the history of all cultures, primitive and civilized'. I take this to be the vision of all devoted ethnologists. Precisely because ethnology is culture history, it will not be thrown to the wolves but will live on in one disguise or another. Even those ethnologists who occupy themselves solely with research on primitive peoples will be needed, not just because there are a few primitives left to be studied in the field but because culture history needs the pieces of information these 'primitivologists' can supply from the field, archives or libraries. We shall, perhaps, face a time when it will be as important to know the culture history of the Bakongo as it is today to know the culture history of the Japanese. In that event, the various ethnological traditions will come closer to one another and, perhaps, finally merge. Their mutual approach will depend on a regular interchange of information, something which can be furthered on the institutional level, through the international ethnological societies (*cf.* Keesing, 1960). We must begin a meaningful debate between representatives of different ethnological traditions based on the awareness of the existence and import of these traditions. In the long run, such an awareness is bound to reflect the realities of intercultural communication of the peoples of the world which as ethnologists it is our mission to foster.

References

Banton, M. (1964), Anthropological perspectives in sociology, *British Journal of Sociology,* 15 (2).

Brinton, D. G. (1892), The nomenclature and teaching of anthropology, *American Anthropologist,* 5.

Erixon, S. (1937–38), Regional European ethnology, *Folklives,* 1–2.

— (1951), An introduction to folklife research or Nordic ethnology, *Folklives,* 14–15.

— (1962), Folk-life research in our time, *Gwerin,* 3 (6).

— (1967a), European ethnology in our time, *Ethnologia Europaea,* 1 (1).
— (1967b), Urgent ethnological tasks, *Ethnologia Europaea,* 1 (3).
Evans-Pritchard, E. E. (1950), Social anthropology: Past and present, *Man,* 50.
Fenton, A. (1967), Historical ethnology in Scotland, *Ethnologia Europaea,* 1 (1).
Firth, R. (1951), Contemporary British social anthropology, *American Anthropologist,* 54 (4).
Freudenfeld, B. Ed., (1960), *Volkerkunde: Zwolf Vortrage zur Einfuhrung in ihre Probleme.* Munchen.
Gjessing, G. (1948), *Mennesket er ett: Kulturforskning og Kulturkrise.* Oslo.
Haekel, J. (1956), *Zum heutigen Forschungsstad der historischen Ethnologie. Die Wiener Schule der Volkerkunde, Festschrift.* Wien.
Haollowell, A. I. (1960), The beginnings of anthropology in America. In F. de Laguna (Ed.), *Selected papers from the American Anthropologist 1888–1920.* Evanston, Ill.
Heilfurth, G. (1961), *Volkskunde. Handbuch der empirischen Sozialforschung,* vol. I. Stuttgart.
Heine-Geldern, R. (1964), One hundred years of ethnological theory in the German-speaking countries: Some milestones, *Current Anthropology,* 5 (5).
Herskovits, M. J. (1949), *Man and his works: The science of cultural anthropology.* New York.
— (1959), Past developments and present currents in ethnology, *American Anthropologist,* 61 (3).
Hoebel, E. A. (1958), Anthropology, universities, and American society, *American Anthropologist,* 60 (4).
Hultkrantz, Åke (1952), American 'anthropology' and European 'ethnology': A sketch and a program, *Laos,* 2.
— (1960), *General ethnological concepts: International dictionary of regional European ethnology and folklore,* vol. 1. Copenhagen.
— (1965a), Anthropology as a goal of research: Some reflections, *Folk,* 7.
— (1965b), The study of North American Indian religion: Retrospect, present trends and future tasks, *Temenos,* 1.
— (1966–67), North American Indian religion in the history of research: A general survey, Parts I–IV, *History of Religions,* 6 (2–3), 7 (1–2).
— (1967a), Some remarks on contemporary European ethnological thought, *Ethnologia Europaea,* 1 (2).
— (1967b), Historical approaches in American ethnology, *Ethnologia Europaea,* 1 (2).
Ishida, E. (1965), European vs. American anthropology, *Current Anthropology,* 6 (3).
Izikowitz, K. G., *et al.* (1959), Trends in anthropology: Anthropology in Sweden, *American Anthropologist,* 61 (4).

Jacobs, M., and Stern, B. J. (1947), *Outline of anthropology*. New York.
Jensen, A. E. (1954), Bemerkungen zur kulturmorphologischen Betrachtungsweise. *Studium Generale*, vol. VII.
Jettmar, K. (1964), Fruchtbare Ansatze im ethnologischen Denken der Vergangenheit. (Reprint)
Keesing, F. M. (1960), The International Organization of Anthropology, *American Anthropologist*, 62 (2).
Kroeber, A. L. (1953), Concluding review. In S. Tax *et al.* (Eds.), *An appraisal of anthropology today*, Chicago.
— (1962), Anthropological horizons, *Current Anthropology*, 3 (1).
Leach, E. R. (1961), *Rethinking anthropology*. London.
Lowie, R. H. (1934), *An introduction to cultural anthropology*. New York.
— (1937), *The history of ethnological theory*. New York.
Muhlmann, W. E. (1948), *Geschichte der Anthropologie*. Bonn.
Murdock, G. P. (1951), British social anthropology, *American Anthropologist*, 53 (4).
Rank, G. (1962–63), Etnologi och historia, *Folklives*, 26–27.
Rohan-Csermak, G. de. (1965), Le probleme de l' 'ethnie' et la notion de 'complexe ethnique europeen'. Manuscript.
— (1967), La premiere apparition du terme 'ethnologie', *Ethnologia Europaea*, 1 (3).
Shimkin, D. B. (1949), Recent trends in Soviet anthropology, *American Anthropologist*, 51 (4).
Tax, S., *et al.*, Eds. (1953), *An appraisal of anthropology today*. Chicago.
Tokarev, S. A. (1967a), Die Grenzen der ethnologischen Erforschung der Volker industrieller Lander, *Ethnologia Europaea*, 1 (1).
— (1967b), Das Prinzip des Historismus in der sowjetischen Ethnologie, *Ethnologia Europaea*, 1 (2).
Vucinich, A. (1960), Soviet ethnographic studies of cultural change, *American Anthropologist*, 62 (5).
Winick, Ch. (1956), *Dictionary of anthropology*. New York.

PART II

National traditions

5 The United States: Racism and progress in the nineteenth century

JACOB W. GRUBER

1. Introduction

Geographically and sociologically, the United States of the nineteenth century was already a nation of considerable size and complexity. Its geographical and political isolation combined with the novelty of its past to produce a cultural atmosphere that one can vaguely regard as 'the American experience', one that perhaps only anticipates the modern 'Western' experience. That particular point of view put its stamp upon politics, upon social structure and upon all of the developing institutions of the new society. It is not surprising, therefore, that the peculiar concerns which cluster around an examination of mankind, those that form the foundations of a self-conscious anthropology, should also be marked by national peculiarities, as they were elsewhere, where political boundaries reinforced linguistic differences and historic rivalries to provide the centripetal social forces which produced national traditions in England, France and Germany. Such a 'national' approach is the more predictable for movements that have their origins in the first half of the nineteenth century, when the chauvanistic nationalisms generated by a deteriorating romanticism eroded the notion of an international science rooted in the universal rationalism of a preceding age. Although we are familiar with the concept of *zeitgeist* and its influence upon systems of thought, we should also be aware of the importance of the *ortgeist*, the cultural context of place, the effect of which has been stressed by Boring (1950, 1956) in his analyses of the history of psychology and the history of science. For psychology, in particular, he notes (1950, p. 179): 'The formative environment was the American

temperament of the late nineteenth-century. A rough, crude, frank, aggressive, practical, boastful nation America was ... the psychologists of that period had no thought that a new insight resided in them. They were looking to Germany for the best and the newest, and trying to see that America did as well or better. They seemed scarcely aware that, in taking over German psychology, they were remaking it along American lines. But America presently did much more. It changed the whole German intent as to the purpose and significance of psychology.'

In this paper, I shall outline two traditions, points of view or conceptual systems which provided the meaning for investigations of an anthropological character. Secondly, I shall attempt to interpret the historical context used.

2. Historical development and traditions

In examining the history of anthropology, it has become commonplace to recognize that the field did not begin–even in the sense of a discipline–with the arrival of Franz Boas in the 1880s or with Lewis Henry Morgan as a lone intellectual beacon flashing the promise of the future – and the reality of the past – in the utter darkness of the present. Whatever Boas' accomplishments – they were monumental, despite the efforts of latter-day critics to denigrate them – and whatever present value may be placed upon the brilliant efforts of Morgan to comprehend the human experience, there was and there had been extensive and systematic anthropological achievement and interest in the New World for at least half a century before. It was upon such a base that the later anthropology with which we are more familiar was built as both a source and a reaction. And it is this congery of interests in man which came to form what I would regard as a national tradition – or, more properly, a cluster of traditions – in anthropological research and exposition.

Before discussing the particulars of such traditions, however, and their effects upon the more restricted institution of anthropological inquiry itself, it is necessary to note briefly the relationship of such traditions to history. The concept of tradition cannot be divorced from the historical and/or cultural context within which it operates. Intellectual traditions have their source in an existing sociocultural matrix which forms the structure of knowledge. Particular traditions mirror the more

general historical frame. They take on meaning and significance from the assumptions, the premises and the goals which insure the continuance of the cultural system as a whole and provide that coherence which makes generational transmission so relatively efficient.

The correlation between the more specialized tradition and the general culture is closer and more direct in those intellectual areas which have relevance to man himself. Where human concerns intrude, where a definition of man is conceived, where there is the suggestion of goals and values, there the effect of the overall cognitive system upon the particular system of inquiry will be the greatest. And certainly this is the case for the investigative or speculative activity which can be subsumed under the term 'anthropology' through the middle of the nineteenth century. It is for this reason that 'national' traditions are more readily discernible in anthropology – or the social sciences in general – than in such 'hard' sciences as geology, chemistry or physics.

It is difficult to justify the separation of the history of science from social history at any time, particularly so in the case of the history of the 'science of man'. But there is, I submit, an anthropological approach to the study of the history of science. It is an approach which, in contrast to the institutional emphasis of the sociologists of science, stresses the affinities of the particular science to the general cultural system of which it is a part. This essentially anthropological perspective – 'historical' in the Boasian sense – makes it essential that the broader picture of the intellectual milieu be drawn so that the outlines of the anthropological traditions are sharply revealed as a figure against a ground. To paraphrase Gilbert Murray's sensible definition of the role of the classical scholar, it is useful to remember that the historian's special duty is to turn the oddments of his research into living thought or feeling. 'He must so understand as to relive' (Smith and Toynbee, 1950, p. 15), and that is the canon of participant observation, as anthropologists will recognize.

The development of American anthropology during the nineteenth century was in a dual tradition. Although aware of and related to the anthropological enterprise in Europe, these traditions were American in their content and historical relevance. They continued throughout the century (indeed, they still maintain their force), and they had a point of view, a bias and a body of data upon which a later self-conscions anthropology was built.

First, there was a consuming interest both linguistic and cultural in

the study of the American Indian. Even after an earlier, and eccentric, interest in the origin and theological relevance of the Indian had declined, he remained as an infinite object of research and speculation. His very presence provided the challenge for a developing ethnology. His existence is as significant in the history of American anthropology as it was in the geographical and political expansion of the new nation, and, of course, both circumstances are related. The dedication to the study of this 'American race' was given official sanction when, in adopting a table of organization for the Smithsonian Institution in 1870, the Board of Regents noted as one example for which appropriations might be made 'ethnological researches, particularly with reference to the different races of men in North America'. Acknowledging a relationship which was to continue in theory more than in fact, they added: 'also explorations and accurate surveys of the mounds and other remains of the people of our country' (*Annual Report*, 1871, pp. 9–10). Government officials and scientists took seriously the obligation to recover and preserve all that was possible of the culture of this race, which seemed doomed to extinction within the new society.

The Indian – in his ethnological present and archaeological past – was a coherent theme. The discontinuity which seemed to exist between him and the European, on the one hand, and the Negro, on the other, suited an ethnological tradition whose focus was the 'nation' or 'race' rather than the species entire.

It was this concern with the American Indian, then, so uniquely a part of the New World, that stimulated early ethnological studies and collections. Reflecting this interest as early as 1812 the American Antiquarian Society and its museum was established. 'Its immediate and peculiar design is to discover the antiquities of Our Continent, and by providing a fixed and permanent place of deposit, to preserve such relics of American Antiquity as are portable, as well as to collect and preserve those of other parts of the Globe' (Shipton, 1967, p. 35). Horatio Hale, Lewis Henry Morgan, Daniel Brinton, along with John Wesley Powell and his colleagues in the Geological Survey and the Bureau of Ethnology pursued the Indian throughout the century 'with the view of securing to Ethnological science available records of races destined ultimately to disappear'. At the same time, men like Squier and Davis, Wyman, Abbott and Putnam sought a reconstruction of that race's past in an expanding archaeology.

These early studies were essentially nonevaluative in nature, and,

although this humanistic view – one which affirmed the unity of mankind – was attacked and became increasingly suspect on 'scientific' grounds, it formed a significant strand of the ethnological tradition. Where they went beyond ethnographic salvage, imposed by a self-conscious ethnology, these studies conceived the Indian as a peculiar segment of the human species: analysis of their history, language and culture would contribute to an understanding of both the differences among men and their common humanity. This implied a search for the nature of the mind, which was so obviously the primary distinguishing characteristic of man. This quest was rooted in the rationalism of the eighteenth century; its success might lie in a knowledge of the essential nature of language. To language the ethnologists of the nineteenth century added custom, but the goal remained unchanged. As late as 1870, George Gibbs (p. 367) could therefore write as an argument for greater efforts to collect Indian linguistic materials: 'How many languages which have arisen since primeval man first trod the earth have perished from its face, each indicating, to a certain extent, in its mere words and forms, not only the outward circumstances in which these races moved, but the degree of their mental culture and the character of their thoughts. Ethnology has no surer basis than an intelligent philology. One of the least functions of the latter is to bring together or to distinguish races.' Similarly Hale argued for the same kind of linguistic primacy in ethnology until his death toward the end of the century (Gruber, 1967).

What distinguishes this particular tradition is the absence of the assumption that ethnic differences implied a higher or lower position on the scale of nature. Whatever their distinctions, whatever the degree of their historical isolation or ethnic affiliations, the various Indian groups were a part of mankind. The unwillingness to magnify differences within the species, to view behavioral distinctions as significant classifcatory criteria, made it difficult for the ethnologist to adopt the ladder of being which was of such importance to the conceptual system of the natural historian. It is this sense of humanity, an awareness of.mankind, which runs through the work of such ethnologists as Morgan, with his enthusiasm for the Iroquois, or Stephens, with his musing on the genius of the vanished builders of the monuments of Yucatan.

This emphasis upon the Indian also provided a logical basis for the union of archaeology and ethnology within the context of a particular cultural, rather than a universal, history. With few exceptions, there was a general recognition that the antiquities so commonly encountered

were only the fragments of a culture that was historically distinct from that possessed by the living people. This linkage between the prehistoric past and the ethnographic present was, in its intensity, peculiar to the American ethnological tradition.

While the American Indian as an object provided American ethnology with methods and concepts that were to be clearly defined, the ethnologist worked within a more abstract ethnological tradition. Its characteristics may be summarized as follows: (1) It assumed a static and unchanging human nature with overtones of social progress and perfectability; (2) it emphasized the uniqueness of man within the natural order, i.e., man apart from nature; (3) it emphasized a comprehensive description of each segment of mankind without invidious comparisons; and (4) it had a Romantic feeling for both the recognition and value of ethnic diversity while at the same time stressing the unity of mankind.

While it is easy to recognize the cultural or behavioral perspective in the works of these nineteenth-century ethnologists, a perspective which stems, I think, from the implicit assumption that the nature of mind is reflected in the nature of behavior, it is necessary to recognize another, contemporary perspective which stimulated another tradition in the description of man in his variety. Related to an older European tradition, which applied the Linnean classificatory scheme to man, it provided scientific justification for descriptive distinctions among the various 'races' of man. Although its practitioners emphasized detailed description of physical variations, they also sought to understand this 'unique' species as part of an organic universe; thus, their criteria, following the Linnean revolution, tended to be biological, actually anatomical, rather than behavioral. In *The leopard's spots*, William Stanton (1960) has reviewed some of the activity generated in this country by the concern for man as a biological species, at a time when the primary goal of 'natural science' was to lay out the fabric of nature on the loom of classification with a presumed Baconian induction as the tool. Arguments and disputes over the *specific* nature of man and his varieties obscured the classificatory issue so simply illustrated, for example, by P. A. Browne's (1850, 1852) attempt to determine the differences between the hair of the white and that of the Negro or, more generally, by the effort to find the single biological criterion that would prove the unity or diversity of mankind. That question had, of course, the same social, political and theological implications (although

initially it was a question of systematics) as it was to have in Europe a century later. The physical proximity of both Negro and Indian – and the social disadvantages which each suffered – led American research into politically racist rationalizations. Moreover, the American investigator, his subject alive and at hand, could easily rationalize the physical as determining the behavioral. The monument to this 'cis-Atlantic school of anthropology' is the first part of Nott and Gliddon's *Types of mankind* (1854), and its culture hero was Samuel Morton. Of him and his work, his eulogist wrote (Patterson, 1854):

> It is evident that the published matter for Morton's studies was very limited. A pioneer himself, he had to resort to the raw material, and obtain his data at the hand of nature. Fortunately for him, he resided in a country where. if literary advantages are otherwise deficient, the inducement and opportunities for anthropological research are particularly abundant. There are reasons why Ethnology should be eminently a science for American culture. Here three of the five races, into which Blumenbach divided mankind, are brought together to determine the problem of their destiny as they best may, while Chinese immigration to California and the proposed importation of Coolie laborers threaten to bring us into equally intimate contact with a fourth. It is manifest that our relation to and management of these people must depend, in great measure, upon their intrinsic race-character. While the contact of the white man seems fatal to the Red American, whose tribes fade away before the onward march of the frontier-man like the snow in spring (threatening ultimate extinction), the Negro thrives under the shadow of his white master, falls readily into the position assigned him, and exists and multiplies in increased well-being. To the American statesman and philanthropist, as well as to the naturalist, the study thus becomes one of exceeding interest. Extraordinary facilities for observing minor subdivisions among the families of the white race are also presented by the resort hither of immigrants from every part of Europe.

Morton's anthropology was a branch of natural history; whatever interests he and his followers *may* have had in elucidating the essential nature of man, all of their researches had as their ultimate goal the awareness of man's place in nature. In the passage quoted above, it is possible to see something of the special character of this approach and the reasons for its enthusiastic reception by American naturalists. Although it shared with ethnology a concern for the description of particular ethnic entities, it was less interested in historical connections or intraspecific commonalities. Its stress upon the biological, differentiating characters in fact led inevitably to the construction of *specific* differences among the races, a view that both *simplified the overall clas-*

sificatory problem and at the same time eased reconciliation with the realities of the existing social situation.

Although the initial concerns were descriptive, i.e., scientific within the canons of science of the period, the natural history approach of Morton's anthropology was always conceptually linked to an older (and somewhat faded) Linneanism, with its implied hierarchical arrangement of specific (i.e., species) forms. The assumed proofs for the distinctness of the varieties of mankind which characterized this kind of inquiry led to the conclusion, long before the popular acceptance of social Darwinism, that there were qualitative differences among the races and that these differences were rooted in the physical nature of the 'stock' and, thus, in nature itself. The scale of nature, thus revised, did not regard man as a single, unified species; the greater precision of the new science ranged man's varieties – translated into species for the occasion – along a continuum of relative perfection. Thus, Browne (1850) could use his microscopic comparative studies of hair to declare that 'the hair of the white man is *more perfect* than that of the negro; and, as we know by experience, that of all pile, that of the head of man is the most completely organized, we will not, perhaps, be wandering astray, in ranking the hair of the head of the white man as *a perfect hair* ... for according to the rules of science, one organ is considered *more perfect* than another, if it employs a greater variety of apparatus in the performance of its function', i.e., if it is more complex.

This kind of scaling, so easy and so reasonable if one considers species rather than varieties, defines that anthropology in this country which grew out of and was a part of natural history. Although still within a world view which stressed the static nature of organized creation–and which, therefore, saw the duty of the scientist as one of description and classification–this view of the meaning of racial differences was compatible with an ethnocentric notion of progress, the only form of systematic change that had any respectability. It was this notion of progress that both Spencerian and Darwinian theories of evolution seemed so admirably to support. That is, the result of such support of a system of organized differences by the adherents of progressive change was, paradoxically, deepened awareness of racial differences and the lending of 'scientific' sanction to the notion that qualitative distinctions among the varieties of mankind were significant clues for the understanding of the order of nature and, by inference, of the divine design.

The reinterpretation of the idea of progress and human perfectability

within the scientifically respectable doctrine of organic evolution gave a meaning to racial differences which the earlier ethnologists, intent upon filling the gaps in a classifactory sense, had not intended. Underscored by the progression observed in the newly revealed strata of human prehistory, the idea – so easily available to common sense and so willingly accepted by common experience – that racial differences represented the various stages of man's evolution (read progress) through the past to the pinnacle of the present became common currency. Primitive man was indeed primitive, for on the long, ascending road which man had traveled, he represented the first, or earliest, products of that vast and continuing experiment in the evolution of perfection. For the support of such a thesis, the comparisons between the relevant culture of the living savage and the implements accumulated by the prehistorian seemed to afford abundant evidence.

It is important to note that this general thesis–an initially naive and innocent racism–was related to the success and popularity of biology or natural history as science. It was biology, of course, which provided the models for the interpretation of human data, as natural history had earlier. In contrast to the work of the ethnologists, whose concern was with the distinctive characteristics of man as expressed primarily in his behavior (an approach that was essentially humanistic), the 'evolutionists', searching for a universal history, saw man as part of a biological and physical nature. In this the Americans mirrored, although with a greater passion, what had emerged after mid-century as anthropology on the Continent and in England. This *anthropology*, so different from a continuing ethnological concern, was characterized by: (1) An evolutionary approach with a concern for species origins; (2) a basic assumption and emphasis upon man as a part of nature; (3) a stress upon process and especially upon progress; (4) a rigid scientism and biological bias, with a consequent exclusion of humanistic and ameliorative concerns; and (5) a concern with racial differences as a means of elucidating the evolution of the species, i.e., a view of races as evolutionary stages. That this approach, nourished by the continuing presence of the Negro in a condition of slavery, was congenial to a changing American society–particularly in the traditional intellectual heartland of New England–assured its acceptance and popularity both as science and as myth.

3. Major influences

What, then, were the major influences that shaped these two American traditions (or dual tradition, if you will) the *ethnological* and the *anthropological*? One significant element is the sharp rejection, which occurs after the War of 1812 and marks the Jacksonian period, of England as an intellectual source. That break, initially political, reinforced nationalism and necessitated the search for native roots. Such roots were by no means as easy to dig up in the New World as were those which folkloristic, linguistic and historical enthusiasts were uncovering throughout the Old. After all, Americans were new arrivals. Their past was a European past, but the return to home and ancestry was barred by the mutual denial of parentage. The intellectual, almost spiritual, isolation which followed from this nationalistic fervor encouraged here – as it did throughout Europe – the use of native institutions and native intellectual interests to form the foundation of native traditions. One can view this emerging nationalism not so much as a break in communications of the sort that accompanied a nationalistic insistence upon the use of the local language for the effecting of intellectual concerns, but rather as the gradual disappearance of the idea of America's intellectual dependence upon England. In extolling the function of the scholar, Emerson in 1837 declared his independence and faith in the American future: 'Our day of dependence, our long apprenticeship to the learning of other lands, draws to a close. The millions that around us are rushing into life, cannot always be fed on the sere remains of foreign harvests. Events, actions arise, that must be sung, that will sing themselves.' Whatever the posture of complete independence, however, the practical effect of the intellectual nationalistic fervor was to open the way to the scientific influences which were emanating from France and, more importantly, from Germany.

This intellectual nationalism was particularly marked in the sciences. The success of the *American Journal of Science* or *Silliman's Journal* illustrates both the popularity and the local emphasis of science. Noting the existence and value of scientific journals in England and France, Silliman, in the introduction to his first issue, wrote: 'In a general diffusion of useful information through the various classes of society, in activity of intellect and fertility of resource and invention, producing a highly intelligent population, we have no reason to shrink from a comparison with any country. But the devoted cultivators of science

in the United States are comparatively few: they are, however, rapidly increasing in number' (Dana, 1918, p. 30). The riches which the unexplored resources of the new country promised its scientists stimulated a series of surveys in the natural sciences during the century and supported a scientific nationalism which was to emphasize the values which lay in the unexploited wealth of the nation.

The Indian was not the least of these resources, and the ethnologists of the century felt keenly their responsibility to collect and preserve the valuable data which seemed so certain to disappear with the extinction of the race. Moreover, the Indian, with the ubiquitous remains of his past and a present that was not too different, provided an outlet for the romantic strivings of the American for an American heritage. Whatever the inequities in reality, the American felt a kinship with the Indian that he did not feel with the Negro, whose foreignness was equal to his own. What the European found in his folk past and what he demonstrated with his antiquities, the American sought in the living Indian and the monuments that his ancestors were assumed to have left. In the Indian, the American found his soul.

But there was also the Negro. While the Indian continued to slip beyond the frontier, a pale shadow of the noble savage unable to cope with the complexities of civilization, the Negro persisted and even seemed to prosper in slavery. If the American found in the Indian a noble self, he saw in the Negro the constant goad to his Christian conscience. The Negro in slavery required a scientific theory which justified his treatment, and, capitalizing on attitudes memorialized in folklore, American science provided that justification. Although the polygenesis which supported the natural history approach in American anthropology had its adherents abroad, it was more immediately relevant in the United States where slavery was a domestic social institution.

The intimacy of the three races was an 'accident' of the American experience which profoundly influenced the developing science of man. Racial differences, whose manifest reality was reinforced by social experience, were intensified by a cultural heterogeneity nourished by the mass immigrations into the eastern cities after mid-century. That this immigration introduced a significant class factor into what had been idealized as a homogeneous, even harmonious, social structure increased the awareness of differences – both behavioral and biological – and of their importance for the definition of significant ethnic units.

Further, there was in the United States a fervent belief in the im-

manence of progress as the animating force within a system of nature which itself was unchanging. The rapid growth of the new nation in all of its dimensions during the first half of the nineteenth century was an apparent demonstration of the reality of progress. Moreover, it was the kind of progress that appeared to reward the Protestant virtues of diligence and frugality. Rewards were for the able; the world was for the taking – but only by him who had the wit and the will. Although progress was similarly extolled in England, the vastness of the New World and the infinitude of its opportunities exaggerated its force. Progress was a faith; it was part of the natural order; it could explain – and excuse – the differences among men.

The events of mid-century, culminating in the trauma of the Civil War and the chaos of its aftermath, disturbed such complacency; the war was the prelude to a reformulation of the American experience. The picture of an agriculturally based social order whose stability and harmony lay in man's tie with nature faded in contrast to the events of which the war was both a source and a result. These included: (1) The destruction of the regional centers of commerce and culture through the development of the railroads and the post, industrialization, the rise of cities and the reorganization of laboring populations; (2) the westward expansion and, eventually, the closing of the frontier–both geographically and psychologically; (3) the increase in immigration forced by the need for both settlement and labor, with a consequent acceleration of cultural and class differences within the society; (4) the rapid increase in population and its redistribution; and (5) the emancipation of the Negro and his introduction into the free labor market.

Changes such as these demanded a rethinking of traditional conclusions about human nature. These new conditions which revealed a new world of diversity to the provincial American – an ethnic diversity which seemed inconsistent with any but the most general view of the unity of mankind – reinforced the idea of an emergent, evolving man whose contemporary ancestors littered the path of progress. This provided a comforting rationalization for the realities of social inequality. Although the ethnological aim of the description of human variation persisted, it was the demonstration of the stages in the ascent of man which was central to a later nineteenth-century anthropological theory which sought, in part, to justify social action. The limitations of that approach, both as science and as policy, led inevitably to its revision in the work of Boas and the establishment of the Boasian tradition.

References

Aarsleff, Hans (1967), *The study of language in England 1780–1860.* Princeton.

Annual Report of the Board of Regents of the Smithsonian Institution for 1870 (1871). Washington.

Boring, Edward G. (1950), The influence of evolutionary theory upon American psychological thought. In Edward G. Boring (Ed.), *History, psychology and science.* New York, Wiley. Pp. 159–184.

— (1956), *Ortgeister.* In Edward G. Boring (Ed.), *History, psychology and science.* New York. Wiley. Pp. 332–333.

— (1963), *History, psychology and science.* New York, Wiley.

Browne, P. A. (1850), The classification of mankind by the hair and wool of their heads with an answer to Dr. Prichard's Assertion ... (Pamphlet, Philadelphia). Pp. 1–20. Read before the American Ethnological Society, 1849.

— (1852), The classification of mankind by the hair and wool of their heads with the nomenclature of human hybrids. (Pamphlet, Philadelphia.) Pp. 1–14.

Dana, Edward S. (1918), The *American Journal of Science* from 1818 to 1918. In *A century of science in America.* New Haven. Pp. 1–58.

Daniels, George H. (1968), *American science in the age of Jackson.* New York, Columbia University Press.

Gibbs, George (1871), On the language of the aboriginal Indians of America, *Reprints of the Smithsonian Institution, 1870.* Washington.

Gruber, Jacob W. (1967), Horatio Hale and the development of American anthropology, *Proceedings of the American Philosophical Society,* 111: 5–37.

Hallowell, A. I. (1957), The backwash of the frontier: The impact of the Indian on American culture. Reprinted in *Reprints of the Smithsonian Institution, 1958.* Washington.

— (1960), The beginnings of anthropology in America. In F. de Laguna (Ed.), *Selected papers from the American Anthropologist 1888–1920.* New York. Pp. 1–90.

Higham, John (1955), *Strangers in the land.* New Brunswick, N. J.

Nott, J. C., and Gliddon, George R. (1854), *Types of mankind.* ... Philadelphia.

Parrington, Vernon L. (1927), *The romantic revolution in America 1800–1860.* New York, Harcourt Brace.

Patterson, Henry S. (1854), Notice of the life and scientific labors of the late Samuel Geo. Morton, M. D. In J. C. Nott and George R. Gliddon (Eds.), *Types of mankind.* ... Philadelphia. Pp. xvii–lvii.

Shipton, Clifford K. (1967), The Museum of the American Antiquarian Society. In W. M. Whitehall (Ed.), *A cabinet of curiosities.* Richmond, Va.

Smith, Jean, and Arnold Toynbee. (1950), *Gilbert Murray: An unfinished autobiography*. London.
Solomon, Barbara M. (1956), *Ancestors and immigrants*. Cambridge, Mass.
Stanton, William (1960), *The leopard's spots*. Chicago.
Struik, Dirk J. (1948), *Yankee science in the making*. Boston.
Wiebe, Robert H. (1967), *The search for order*. New York, Hill and Wang.

6 Franz Boas: The academic response

GENE WELTFISH

'... scientific activity alone is not enough; I must be able to livingly create.' Franz Boas

1. Franz Boas

If we speak of the Boas tradition, we speak first of Boas the man as he stands out in living memory as a teacher and scholar. My first encounter with Boas was in 1924 when, as a senior undergraduate, I entered his class in General Anthropology. One hundred women students had squeezed into the room in Columbia University's Barnard College. Incidents connected with his German background had deprived Boas of the male counter-part of our Barnard College class in Columbia College during World War I (Knoeber, 1943, p. 15). He was 66 years old, and a serious operation on his cheek for a cancerous growth in 1915 had distorted one side of his face and caused a speech impediment. His head was bald except for a leonine graying mane at the back. These were the stark facts, but the eyes typically twinkled or flashed with anger, indignation or interest as he set before us a vision of the stream of humanity moving across Asia into Europe – shown not through ordinary words but with slides of Bronze Age swords, pottery and fibulae, whose typologies and stratigraphies revealed the whole story; the idea shown through the object – to develop the broad panorama of human experience. This motif had been a principle theme in Boas' thinking for a long time.[1]

1. Among other sources of his interest in archeology, it should be noted that Virchow, Boas' teacher, had worked with Schleiman at Troy in 1879, an inspiring validation of old Greek literary sources (Kluckhohn and Prufer, 1959, p. 5). An important outcome of these archeological interests was the founding under Boas' direction of the international School of American Archeology and Ethnology in Mexico and his establishment, through the stratigraphic method, of the fundamental elements

of the sequence of cultures in the Valley of Mexico, worked out substantively with Dr. Manuel Gamio (*cf.* J. Alden Mason in: Kroeber, 1943, pp. 59–61).

As a graduate student applying for admission, I had been ushered into Boas' office for consultation. There was a general discussion of how long and difficult a process it was to become an anthropologist and how hard it was to earn a living. 'Why', asked Boas, 'do you possibly want to do this?' I said that I had always earned a living one way or another and that I was prepared to continue to do so if I could also continue with what I felt was my real work. The next question: What courses had I taken at Barnard College? General Anthropology, Zoology, Philosophy, German, Mathematics, English Literature and Greek Art. Boas had some letters of recommendation before him. 'Greek Art! You will be interested in art.' It was a surprise to me, as I had taken the course for the man (Clarence Young) not the subject. I didn't see at the time how definitely he had decided. Maybe Boas was right, although later on I fought him. If I had wanted to pursue my initial interest in epistemology, I should have stayed in the Philosophy Department.

I should like to return to this classroom of the 1920s, this time in linguistics. My first experience there was unusual. Boas sat at the table in front of the room, and most of the students were ranged at the blackboards, writing something in a wierd hieroglyphic script. Boas was making a series of clicks in what I was given to understand was indeed a language, later identified as Kwakiutl, and the script was the International Phonetic Alphabet. I was directed to take my place at the blackboard with the other students and, while I protested that I knew nothing of what it was all about, Boas commanded, 'Copy from your neighbor.' Then Boas came to the board and corrected each of our written texts in turn, mine included. Some years later, when I came to teach practical linguistics, after my own work in the field, I was determined that I would not subject my students to such an experience. For three weeks I eased them gently into phonetics via recorded English and then to work with informants. The next year, I felt that one week of such preparation was more than enough – possibly even a waste of time – and in the following year I heard myself saying as the students stood ranged along the blackboard, 'Copy from your neighbor'.

In the graduate school, Boas' presence struck awe in our hearts. Once, a small group of us, including Alfred Kroeber, then a Visiting Professor, was leaving a faculty meeting. Boas was in the lead. Kroeber, wanting to say something to 'the old man' stood diffidently off to the

side, his hat held behind him. I whispered to him, 'You *are* a famous professor now, you know'.

For some of his former students that aura became an active hostility. One of them was Edward Sapir, whose monogenetic theory of the origin of language ran counter to Boas' polygenetic view. Sapir, with almost painful sensitivity, sympathized with a young student whom he felt Boas was treating abstractly rather than humanly. As Sapir put it, 'He's always trying to put square pegs in round holes'. Beyond that, however, for all of us Boas was a teacher who shared generously with us his own intellectual quest.

The Wednesday departmental seminar from four to six was the big proving ground. Boas did not talk down to people, nor did he coddle them. He selected the subject and developed it, assigning to each graduate student a topic, with the date the report was to be given. I drew the first topic, 'Parallelism and convergence in culture', due in three weeks. Since it was a famous seminar in which the Boas 'school' was being formulated, it drew other faculty and outsiders who joined in the discussion. The new graduate student had a right to be apprehensive. Dr. Cecil Yampolsky, on 'Parallelism and convergence in plants' and Dr. McGregor, of the American Museum of Natural History, on 'Parallelism and convergence in animals' preceded me. As I stood at the podium, Ruth Benedict, sitting in the front row, nodded her encouragement. After introducing me, Boas joined the others in helping me bring out the essential points.

We were encouraged at every opportunity, and especially at professional meetings, to deliver papers. Although we did not get over our nervousness, it lessened in time, and we learned how to do what is certainly one of the most essential aspects of an academic, and even scholarly, career. Boas believed that delaying by extensive preparation is not as effective a strategy of learning as is direct action. He also praised and encouraged us as much as he could in those initial stages, sometimes deliberately provoking us by making an incomplete or inaccurate statement. In one of his seminars he was characterizing Plains Indian art by its angularity. When I raised my hand and pointed out that circular rosettes were also a favorite decorative device, he said, 'I hadn't thought of that'. I felt fine. I mentioned this to John R. Swanton, who told me: 'Boas gave me a Tlingit vocabulary and after looking it over, I pointed out some regular phonetic shifts. It was obvious that the words were arranged so that you couldn't miss it. The result was electric. Boas called

out to his secretary, "Miss Andrews! Miss Andrews! Dr. Swanton has just discovered a phonetic law!".'

Another anecdote occurs to me as particularly revealing of Boas the man. I have never been much of a letter writer, and at one time when I was moving around the Southwest, a telegram from Boas caught up with me. It said, 'Write to your mother. Franz Boas.'

Boas was trained as a German scholar, with all the concomitant intellectual discipline and educational background. Born in 1858 in Minden, Westphalia, he attended the State School *(Volkschule)* for four years, the Gymnasium at Minden, and then the Universities of Heidelberg, Bonn and Kiel, where he took his doctorate in 1881. Before he was 13, he had been exposed in intensive, formal study of botany under a special tutor. Although he found mathematics very difficult, he was determined to master it, and it later became his most absorbing preoccupation. He never lost his love of mathematics and, in the 1920s, as he commuted by ferry from his home in Grantwood, New Jersey, he enjoyed long, theoretical discussions with his accomplished colleague in mathematics at Columbia, Professor Cassius Jackson Keyser. In his *curriculum vitae* upon his graduation from the Gymnasium at Minden, he mentions having studied botany, geology, mathematics, geography, English, biology, minerology, Greek and Latin. These preuniversity interests found expression in the conduct of his classes at Columbia. The geography that formed a major part of the German curriculum included anthropogeography, one of whose chief exponents was Ratzel. Here was the beginning of the panorama of human migration that was set before us in our Barnard College class.

One of his most head-twisting courses in the graduate school was anthropometry which required a combination of statistical and anatomical skills. This course had its origin to some extent in his contact with the distinguished pathologist Virchow (1821–1902), who had been his uncle's teacher. 'Virchow was the first eminent nineteenth-century German to argue for the intrinsic equality of the races and his publications on this subject caused an uproar in the Second Reich' (Kluckhohn and Prufer, 1959, p. 22). Virchow demanded adequate empirical substantiation of theoretical generalizations and insisted that students not only be instructed in techniques but also gain practice in sound research practices. Virchow was the only man of his generation to uphold the plasticity of races and the possibilities of change in cranial indices. How strongly Boas was influenced by Virchow in this field is shown

by his paper on the relative merits of the length-breadth, length-height indices of the skull and the effects of environmental influences upon growth processes in general. If environmental conditions affect these measurements, they can hardly be used as a criterion for long-term racial identity. This revolutionary insight into the use of statistics has been followed up in later research.

Boas was in the forefront of the development of new techniques of biometry, both adapting the statistical methods of Pearson and originating new ones (Tanner, 1959, pp. 77–78). Not only did he combine statistical and anatomical methods to make a lasting contribution to the field, but he was also keenly aware of their political implications for combatting racism. In his three general books, *The Mind of primitive man* (1911), *Anthropology and modern life* (1928) and *General anthropology* (1938), his statistical findings had a prominent position, as he felt that man's biological make-up should take priority in the study of humanity.

The classroom was, of course, only ancillary to the real work of the graduate student. For Boas, the collection of linguistic texts from native informants was a rescue operation; if we were ever to consider the human mind in its manifold versions, these texts were irreplaceable; insofar as possible, such a resource must be converted into published form. The academic management of such an enterprise was a highly demanding task. It was a costly and unpopular 'antiquarian interest' to the usual agencies granting funds. Indeed, the potential use of these texts to expand our insights into the varieties of human conceptualization has not yet been realized. As we strive for an improved quality of life, we will move into a level of experience for which we have no precedent. We will then have to couch our experiences in terms and metaphors of which at present we have a considerably limited array compared with those available through the study of more primitive peoples. The use of self-serving semantic generalizations by the military, the political office holder and the commercial seller and advertiser as a shield against their constituents is only too patently on the increase today. We all need much keener semantic sophistication as a counterbalance to these attempts to distort the facts by linguistic legerdemain.

This is complicated by the fact that the languages now spoken by the peoples from whom these texts were originally collected are totally different. I am aware of this from the contrast between the Pawnee language that I recorded in the late 1920s and early 1930s and the language that

I have heard much more recently. The mode of expressing fine distinctions through grammatical devices has diminished in favor of lexical methods, as is characteristic of our developed languages. In a larger community a considerable economy of motion is necessary in communication that covers a wider range of people, lexical redundancy taking the place of grammatical subtlety. As a structure and as a way of expressing human thought, the old Pawnee language is unique; it is highly polysynthetic. A whole thought was integrated into one long word-phrase unit. Today, the Pawnee, all being fluent speakers of English, can produce only the shortest kind of word units. And the universe of discourse of which these polysynthetic units were a function is, of course, today the most meager of traditional fragments (Weltfish, 1936, pp. 44–75; 1958, pp. 301–311; 1965).

Many of the texts were recordings of traditional myths and tales. An interesting treatment of these by Boas (1948, pp. 407–424) can be found in the introduction to Teit's *Traditions of the Thompson River Indians of British Columbia.* After a brief sketch of the geographical situation and the general style of life, Boas compares the treatment of the 'Trickster-Transformer' among three Northwest Coast tribes–the Thompson River, the Tsimshian and Tlingit and the Kwakiutl. These peoples are all conscious of the fact that changes occur continually; they tend to attribute their present situation to changes that have occured in the past. A common explanation is that in the past a mythical personality, in events that marked the course of his life, has in some instances benefited man, although as a wholly unintended by-product, while in other instances he has failed to do so. Progressively, however, in the socially more complex Kwakiutl, some of the acts of the mythical being are performed with a measure of friendly intent toward man, through the friendship with the ancestor of a particular clan, while among the least socially organized Thompson River people, the value to mankind is an entirely random factor in the events of the Supernatural's life. Boas suggests that when a people has conceived of the notion of a friendly Being, they no longer tolerate the changing of conditions as the work of a single 'Transformer'. Instead, they elevate one extant character in their mythology to the position of friendly benefactor, while they regard others as completely self-centered. An unfailingly altruistic prophet, Boas maintains, is probably a unique creation of the most complex civilizations. Therefore, in reporting mythologies, we must guard against reading in our own interpretations by recording,

if possible through literate native speakers, the myths directly in the native language.

I was anxious to go into the field to do some linguistic work preparatory to my thesis but was always held back by lack of money. I had to reconcile myself to writing my thesis about something that was at least a direct product of an exotic people, museum objects. In pursuit of my subject, I walked around the halls of the American Museum of Natural History for hours. I knew from Wundt's *Volkerpsychologie* (Vol. 3, *Die Kunst*) that questions of aesthetic theory were involved in the study of primitive textiles. Finally, I decided that I could better reconstruct the manufacturing processes in coarse weaving than in objects of plastic materials, where the processes of construction had been more completely obliterated. In this instance the designs were an intrinsic part of the textile production. I discussed my thesis with Dr. P. E. Goddard, then curator of anthropology. He said he would speak to Boas about it, and in a week he came back with the topic, 'Technique and design in North American basketry'. Goddard made all the facilities of the museum available to me; the Museum of the American Indian, the Peabody Museum in Cambridge and the University Museum in Philadelphia were equally generous. I handled, recorded and made models of every relevant specimen in an attempt to reconstruct the process of manufacture.

I had given a well-received paper at the annual meeting of the American Anthropological Association, December 28, 1928, at the American Museum of Natural History. The subject was 'Prehistoric North American basketry techniques and modern distributions', and the paper demonstrated certain clear continuities between ancient technological materials and modern tribes. It was evident that these technologies were very old and complex. Since clearly there were many diverse mechanical problems with alternative solutions possible, each of these technical styles had to represent a historical complex. It became clear to me that simply placing the material in tribal and geographic distribution and trying to derive independent variables through tribal comparisons did not produce historically valid units of analysis. As Boas had taught us, you had to place yourself in the position of the maker in order to truly reconstruct the events of manufacture and their historical significance. In the coarse textiles with which I was dealing, this was quite possible. I was anxious to go into the field to check the mechanical independence of my analytic units, which were habits of the maker, not merely the objective characteristics of the object.

It was also possible that conventionalized sets of motions or motor habits were not necessarily limited by the mechanics of the technique itself. In this case, the artist-craftsman was in the realm of 'dance', so that the aesthetic value to the artist was apart from the exigencies of the mechanics of manufacture.

After successfully defending my thesis in May 1929, I at last set out for the field with a fellowship from the National Research Council to examine museum collections throughout the country. One question I posed for the National Research Council study was that in the thousands of objects I had examined and analyzed, there were no duplicate designs even within the rather limited tribal design styles; how was such creativity taught within the tribal context? Visual perception must, of course, play a part, but I also thought that the rhythm of the work might be significant. I was fortunate to be able to put this question to some of the finest artist-craftsmen among the Pueblo and Apache peoples of the Southwest.

At my continued insistence that I wanted to go into the field to do linguistic work, Boas finally managed to find $100 apiece for me and for several other students. He suggested for my project the Caddoan language family, on which we had no systematic field material. For ten years Boas had been attempting to induce the prominent linguist, Bloomfield, to go to Oklahoma in order to work on the language, but he was reluctant. In my case, therefore, Boas thought he could kill two birds with one stone. I was to go to the Haskell Institute for Boys in Lawrence, Kansas, and work with some of the Pawnee students there. However, I was determined to go to Oklahoma. Just at that time the 101 Ranch Circus came to New York and camped in the Polo Grounds uptown. The newspaper account stated that there were 200 Indians, so why not Pawnee? Early one morning I went to the Polo Grounds. Everyone was tired from the night's performance, but a Sioux Indian did turn up with a Pawnee named Henry Moses, who agreed to work with me on the language the next day. We set up a meeting at the Museum of the American Indian. My Pawnee field work began right there and continued for a week. We parted, Henry Moses having dictated a letter to his father that I recorded in International Phonetic Script without any inkling of the meaning. He had been away from Pawnee, Oklahoma, for six years, and, when I did get there and read my phonetic rendition to 'Old Man Moses', it was wonderful to see his excitement.

At Haskell Institute, where I went as directed, the boys had all been

sent home because the buildings were being repaired. Obviously, my next stop had to be Pawnee, Oklahoma. I managed to record some texts and make some grammatical notes along with an analyzed text. I returned to New York via Chicago, stopping off to see Edward Sapir. He was distressed over the fact that I had been used and took me to see Bloomfield, who generously agreed to be my mentor as I continued with the work. Back in New York, Boas was very angry. He told me that first field work was not of much importance. I argued that Sapir had thought the material good and that I had no intention of abandoning the project. Surprisingly, the following year I had two grants: one for the art project and one for linguistics.

Our whole training in the Anthropology Department endowed us with the general sense of a common humanity. In going into the field, therefore, we were not moving into the exotic. As for myself, there was nothing 'strange' or 'exotic' about my field experience; it was intensely human. The problems I took with me to New Mexico, Arizona and Oklahoma produced in me an eagerness to find out. I managed to explain my wants – at first through an interpreter, commonly a school child, or through a combination of simple words and motions – with no difficulty. The people who worked with me either had a native lexical interest of their own or were artists. After we had become well acquainted through my linguistic studies, I undertook general Pawnee ethnography, receiving a traditional designation as tribal historian. Since I was working with the oldest monolinguial informants, the equivalence was made. As we gained insights through sharing, an affective bond developed between us, thus mitigating any possibility of 'field hysteria'.

One of the most serious cases of 'field hysteria' was that leading to the death of Henrietta Schmerler, a young woman of 22, who left our department, under the tutelage of Ruth Benedict, for the White Mountain Apache of Arizona. The country in the White Mountains was wild and desolate; it was rumored that anyone could hide there from the law and lose himself quite comfortably for life. Arthur Woodward, a fellow anthropologist from California, told me of meeting Henrietta at an Indian ceremony and of her fright at being in the field. I think Henrietta had a vague notion of becoming famous through paralleling Mead's Samoan studies, and I was surprised when I heard of her death.

Dr. William Jones, a Fox Indian who had a Ph. D. from Harvard, was killed by the Bontoc of the Philippines among whom he worked.

The strengh of the affective bond was such that they couldn't bear to lose him. As he was leaving, they threw a spear through his chest, thinking by this means to keep his spirit among them.

Boas contributed a lecture to the 1908 Columbia University series of 22 popular lectures on contemporary achievements in science, philosophy and art. It contains many valuable insights into Boas' thinking and some interesting prognostications about anthropology as a science, but most clear is the order of presentation of first the *biological* and then the *mental* aspects of the subject. Boas contrasts these two aspects of anthropology as follows: In physical anthropology, differences *between* human types were the first to attract attention, whereas in ethnology, the *similarity* in cultural types found in remote regions was the first to gain notice. Boas contrasts the general evolutionary-historical approach to the mental aspect of human culture with the psychological approach of Bastian and Dilthey, who asserted that there are elementary ideas implicit in and setting limits to the thinking of every variety of man (Benedict, 1934, p. 47), leading to the presumption of parallel development. Boas counters both of these conceptions with evidence of rapid diffusion of cultural ideas and traits and with the fact that not all evolution is from simple to complex, which is the impression one obtains from a superficial overview of technological development. Language, on the other hand, follows an opposite course. The apparently 'simplest' cultures express minute differences in points of view by means of grammatical categories, while in Latin, and even more so in English, many of these subtler grammatical categories are replaced by simpler and more general grammars. In Chinese, grammatical devices have been abandoned altogether, and the language operates entirely within a lexical framework. 'It is remarkable to find that these [grammatical] categories, which can be discovered only by analytic study of languages, and which are unknown to the speakers of these languages, although they are constantly used, coincide with categories of thought which have been discovered by philosophers. It would be possible to find in the languages of primitive peoples grammatical forms corresponding to a variety of philosophical systems; and in this we may perhaps recognize one of the most brilliant proofs of the correctness of Bastian's and Dilthey's theory of the existence of a limited number of types of thought' (Boas, 1908). A similar phenomenon is to be observed in the art and music of primitive man, where complexities of rhythmic structure are so great that the art of a skilled virtuoso is taxed in the attempt to imitate it.

In the early 1930s the problem of the nature of art occupied a year-long seminar. Boas' book *Primitive art* had appeared in 1927, and in 1928 a volume of the *Annual reports of the American Bureau of Ethnology* was *Coiled basketry in British Columbia and surrounding regions*, an extensive joint effort including the work of Haeberlin, Teit and Roberts. The proofs and photographs were spread all over the Anthropology Department office and in every available space. According to the theory of Gottfried Semper, the architect and art historian and a forerunner of the Bauhaus School, a study of coarse textiles of this type could shed light on the primary sources of art and design in the context of craft-technical activities. Although this concept was developed by Semper in connection with archeological and architectural data, there were also current the psychological ideas of Wilhelm Wundt, who had utilized the observations of basketry techniques among the Amazon tribes made by Max Schmidt, von den Steinen and Koch Grünberg.

Wundt's emphasis on the maker rather than the spectator was consistent with his idea of the contrast between folk psychology and our own. In our completely specialized society, the artist is not of the folk; he represents a totally separate elite. Among tribal peoples, and certainly our ancestral folk, the art world and the world of daily life – the artist and the people – are one. In our society the people are not even spectators except as the arts are finally reduced to popularizations that have been thoroughly sifted through a long series of commercial levels. Wundt reasoned that for art to be so universally engrained in the life of the folk, there must be implicit in the human being himself a primary aesthetic dimension. Wundt deals with the nature of fantasy and observes that no fantasy can produce anything absolutely new. Rather, creativity consists in the ability to reproduce what was once experienced in a *changed order*. Thus, fantasy can be defined as a special, freer form of reproduction, a combination of observations into a new and unique whole.

While fantasy exists in all fields of experience, says Wundt, it has its special form in art. He develops the subject in terms of two areas of fantasy: space fantasy and time fantasy. Wundt observes that three factors are operative in both types:

1. The objective impression – a constant factor
2. Pure memory pictures – an occasional factor
3. Speech, sounds, personal motions or musical rhythms – the repetitive features.

The ever-present elements of the fantasy picture – agitation and rest, boundary and release – shifting feelings and, above all, rhythm, which has power over us and which we in turn control intermittently. This shifting sense of controlling and being controlled by rhythm is most readily exemplified in breathing, whether unconsciously operating or consciously regulated. Thus, rhythm is an objective structure *and* a subjective transformation of that structure.

Wundt counts space fantasy the poorer, since it must draw on the external material. Time fantasy, on the other hand, has its basis in the sounds of speech and the rhythms of the body, factors which the person controls in terms of his own will, independently of external circumstances. The two media – visual art striving toward internalization and musical art striving toward externalization of subjective feeling – are similar in that outside impressions are absorbed, and the movement stimulated by them is then re-externalized. Thus musical art mirrors the manifold forms of the external world in terms of human feelings given expression in natural movements, speech, song, poetry and music.

Boas was clearly in agreement with the vital connection of art with the human organism. In *Primitive Art*, Boas speaks of art as the performance of the artist in its most physical aspect, of the automaticity of technical and technological skill and of the emergence of art in terms of the full control by the artist of these techniques. In Boas' work, however, these aspects are overshadowed by the anthropological dimension, i.e., the insistence that art can be looked at only within the larger cultural context. Boas had sufficient awareness of the unique and holistic elements of a people's 'psyche' to give primary value to their cultural identity. As contrasted with Wundt, Boas included in his treatment of primitive art numerous photographs and drawings of material objects. The phenomenon should speak for itself.

In his chapter 'The study of geography' (Boas, 1948), Boas contrasts Comte's view of the sciences which have as their aim to deduce the laws of phenomena with Humboldt's *Cosmos* whereby every phenomenon is considered worthy of being studied for its own sake. 'Its mere existence entitles it to a full share of our attention and the knowledge of its existence in space and time fully satisfies the student, without regard to the laws which it corroborates or which may be deduced from it' (Boas, 1948, p. 64). He does, however, admit the possibility of deriving general social laws. 'The origin of every science we find in two different desires of the human mind – its aesthetic wants, and its interest in the

individual phenomenon ... The more clearly all phenomena are arranged, the better will the aesthetic desires be satisfied, and for that reason the most general laws and ideas are considered the most valuable results of science.' Implicit here is Boas' rejection of scientific generalizations as the ultimate state of knowledge about the world.

In light of his criticisms of Wundt, Boas encouraged his students to probe the subject further. According to the thesis topic that he had sent to me via Dr. Goddard, I was to investigate 'The interrelationship of technique and design in North American basketry'. For the next three years, I never had any further discussion with Boas directly on the subject, but in the departmental seminar on art, in which I participated, pertinent questions were suggested. Boas disagreed with Wundt on the sequence of developmental priority but not on the importance of the organic basis of rhythm in art. He felt that dance, which Wundt had maintained was the earliest form of art, could not be given priority since it combined rhythm, the time element, with space, the later of the two fantasy areas. Boas was even more critical of Karl Bücher, who maintained that the regular movements in communal work were a source of rhythm. Boas argued (1938, pp. 593–594) that they could not be an original or primary source: 'The pleasure given by regular repetition of embroidery designs, painting, or the complex arrangement of strings in rhythmically repeated order cannot be explained by conditions imposed by the technique and there is no indication that this rhythm is later than the one determined by motor habits [in manufacture]. The great variety of forms in which rhythm appears as rhythm of time in music and dance, rhythm of space in decorative art, shows that the theory of Bücher, who derives rhythm from the regularity of movements in communal work, or the parallel theory of Wundt, who derives it from dance, cannot be maintained.' This interest stayed with Boas, and on his last field trip to the Northwest coast, he brought back a considerable body of film on the detailed foot motions of the dancers. By this time his daughter Franziska had begun an intensive study of dance rhythms and percussion, a field which she has continued to pursue until the present time.

I did not find Boas' criticisms convincing. The real issue is the one that Wundt had expressed – the pleasure in rhythms in art being induced by the physiological rhythms of the artist and, secondarily, of the spectator. I did not feel that we had evidence to settle the priority of either dance or communal work in expressing the externalization of

these rhythms; certainly, both were potent sources. My research was to be centered on craft manufacture *as a source or limitation* upon aesthetic expression. In this analysis I felt it was essential to place myself in the position of the maker and try to reconstruct the process from this point of view, as well as from a consideration of the resulting product. Any technology is a composite of alternative solutions to the many mechanical problems that are involved in the craft. Comparative study of their tribal expression revealed in detail the nature of the variability of each of many minute technical features from tribe to tribe of the same craft object. The question was: With what aspects of the design could such variables be associated? It must be remembered that in coarse textiles, the production of the design is completely identified with the weaving process itself. Which is predominant in any instance, the design image or the design-producing weave? This was to become a central problem in my study of the origins of art (1953).

My studies with artist-craftsmen in the field brought to my attention another element that bore directly on the question of work rhythms and creativity. The motor procedures by which a technological result was achieved were standardized from tribe to tribe, just as was the technological 'style'. The problem I posed to my informants was: 'I have examined thousands of baskets and a great many made by the San Carlos Apache. I have drawn the designs and in comparing them have found that there are no two exactly alike. When we teach our children in school how to make things, they tend to look at the work of one another, and there is a striving to copy what someone else has done. We would consider your way of teaching much more successful than ours. I would like to see how such teaching is done so that we can benefit from it.'

Mrs. Annie Rogers of the San Carlos Apache in Arizona was one of the most accomplished basket makers. She did not speak English at the time, and initially we preempted the translation services of a local Apache school boy. After his explanation, Mrs. Rogers said, 'If you want to know how learning occurs, then you yourself should learn. These are not arts we teach to children. We teach them to our young women when they are to marry. You are not so much older than they are. Sit down now and learn and so you will find out.' I never achieved any virtuosity, but I did put my hands on the work and understood in an elementary way 'the body knowledge' that one must have in order to be an artist-craftsman.

As I moved from tribe to tribe in the Southwest and later in Louisiana, I found that the series of motions and postures involved in these operations were not entirely limited by the mechanics of the task at hand. In addition to the cluster of technological devices that a tribe had developed to produce an object and the design traditions, there was a traditional set of motions, independent of both, almost a 'dance of manufacture'. For example, two Apache tribes that made exactly the same product in all of its technological details accomplished this result with a different set of traditional motions. Among the American Indian artist-craftsmen with whom I worked, this 'dance of manufacture' was of great independent value. I demonstrated to one tribal group the more efficient, motion-saving method that I had learned from a neighboring tribe. It was sincerely tried out and well, but soon there was a return to the traditional set of motions. Not only 'body knowledge' in connection with producing the techniques and design but also the organized movements themselves were part of the art of the artist. In our homely art of knitting, we also have two sets of motions from two different traditions–the German and the English. This approach to alternative motor habits, it should be noted, was used by Gilbreth in his early studies which attempted to alleviate the fatigue of the industrial worker and the physically handicapped.

In our seminars at Columbia my report on the interrelationship of the mechanics of various weaves, the motor procedures of manufacture and their evident effects on design generated great interest in Boas. But by the time I was ready to present my further findings on motion traditions at the American Anthropological Association meetings, Boas was no longer alive, and there was no one left in anthropology who had any inkling of the significance of what I was talking about.

One of the chief postulates of our science is the functional independence of race, language and culture. The placing of a people in one category does not imply their placement in any other category. This distinction was implicit in the thinking of Bastian, who believed that racial types do not determine culture and who rejected language as a classificatory criterion, although admitting that linguistics provides clues to the discovery of historical influences and psychological laws (Kluckhohn and Prufer, 1959, pp. 18–19).

With the rise of fascism, Boas, who had taken such an intense interest in man's aesthetic life, felt that he had to return to more immediate political questions, primarily racism. He had, of course, been deeply

responsive to the revolutionary spirit of 1848 in Germany and had counted among his teachers and intimates Virchow, Bastian, Jacobi and others who had been part of that movement. He lectured, in response to this new crisis, throughout the country. One would have thought that the rational and technical steps he followed in treating the subject would have turned away popular audiences. Instead, people everywhere listened intently. Whatever the terms or the images, they felt that what he said was real and eminently relevant. Sometimes a group of students, including myself, would accompany him, especially to such places as Yorkville in New York City, where German Nazi elements were vociferous when he spoke. He continued to teach his course in anthropometry, which he felt was essential to this question, and another in methods, so that reason would be the framework for our approach to this and other public questions.

The Mind of Primitive Man (1911) and *Anthropology and Modern Life* (1928) were intended as popular presentations. *General Anthropology* (1938) was designed not as a popular rendition of the subject but as a teaching source. Each chapter is full of technical details, and no attempt is made at a clear, logical continuity, although the whole course of human history is implicit in the order of the subjects. Boas, following the injunction of Virchow, insisted that the student should be presented not with unproven generalizations but with the methodological tools with which he might continue to test every theoretical projection.

Following Boas' death and the termination by the sex-prejudiced Columbia University administration of Ruth Benedict's chairwomanship of the department, Ralph Linton, one of Kroeber's former students, was designated to take over the reins. The shift in interest from Boas' direction is perhaps best symbolized by Linton's arranging for the Anthropology Department to be shifted from the Faculty of Philosophy and Humanities, in which it had been for years, to the Faculty of Social Sciences. Kroeber (1943, p. 35) has observed:

> This stream of social science thought passed Boas by without much affecting him; he was nearing seventy and had in hand much of his work to finish; if he was to be distracted–fighting Hitlerism and hate was the nearer job. Thus I see natural history and science and humanistic study as the main factors that actuated Boas' thinking as I see them also in the older anthropology of which Lowie and Sapir and I were part. Before Boas, in England, Tylor was certainly a thorough natural scientist in orientation–and like Boas, his genius seized upon culture as that aspect of humankind most readily susceptible of generalizing phenomenal treatment. ... Contrariwise,

> social science was sired by eighteenth-century *philosophes* and their successors and mothered by hope of practical applicability. It has considerably and consciously patterned itself after natural science; of which the name 'behavioral' is a fair enough expression in a maturer phase. But even behavioral science is still often ameliorationist, taking utility for granted, is definitionist more than phenomenally directed, and, outside of anthropology, it remains ethnocentrically weighted if not aligned.

It would seem from Kroeber's analysis that this shift in direction leaves little hope for a unified anthropology. Developments in epistemology negate this view. Human action is organized around man's image of himself; his directions and his goals must find expression in the form of symbols of man's own making, which extends to the creation of scientific symbols as well.

> In the nineteenth-century, science–whether it consisted in the depictive integration of phenomena or in the formulation of abstract regularities–was 'discovery'. That is, the 'reality' was there, and the task of the scientist was only to find it and order it as it existed, ordered in the world of nature. The twentieth-century scientist regards himself more as a 'disciplined inventor'. That is, he creates models or constructs from experience which he does not necessarily regard as faithful portraits of nature. Schlick has put it that science is not a picture of the real world but a symbolic system by which our experiences can be correlated to each other in a practical way (Kluckhohn and Prufer, 1959, p. 24).

In the past, materialism was the rock upon which we built our stability. The physicist, in full confidence, continued to analyze the physical world, producing ever smaller units of analysis, hoping to get down to the 'ultimate' unit and the most basic building blocks of all that exists. This illusion has been shattered. Under an ever-increasing number of categories, with more and more subcategories, each with properties which defy our capacity to name or describe them, the physicist has now appealed to the philosopher. I faced the same dilemma in my attempt to find the smallest, or final, analytic unit of technological procedures as it correlated with the emergence of the design idea. It seemed to me that only the gross phenomena actually shed any light on the interrelationship between technique and design.

The image that the physicist had concocted of the limitless divisibility of matter has proven false. He was probably creating new units as an artifact of his research methods, not discovering units of nature. As Grace de Laguna (1966), among others, has pointed out, everything in the world occurs within a context from which it cannot in reality be

isolated, and variability is the very essence of the microcosm. Our attempts to get linear causation from a string of isolates may be a convenient fiction, but it does not define reality. Our task will be first of all to understand the image-making process of the human mind. Will we now be called upon to determine not only what human beings do but also why and to what end? What goals will we project for ourselves and for humanity? And with what imagery will we phrase them? These are the questions that motivate the Boas tradition in American anthropology.

> We can only be struck by the limitations of our symbols. Boas indicated that to clarify these limitations, it is important for us to look outside of our own tradition to those of peoples in the furthest corners of the world. The mythologies, the art, the languages of the peoples, recorded when they were more integral to cultures further from the present world stream can be a valuable resource. In the first forty years of this century, Boas collected and supervised the collection of relatively large amounts of archive materials and saw to their publication whenever possible. The material becomes the more invaluable as our understanding of the value of human diversity deepens.

Our vision of humanity moving across the screen of history and the reconstruction insofar as we can and with increasing skill of its course and its detail is a keystone of our consciousness that humanity is one. It is more than knowledge; it is concern. Boas' concern was consistently expressed in his public actions as well as within the science itself. It is in these terms and in the Boas tradition that we have General Anthropology.

The present tendency to be frightened by the increasing knowledge in the ancillary sciences tends toward the teaching of race, language and culture as separate fields of graduate study. We need training in skills, but we also need collaborative, multidisciplinary operations. The fact that the individual scholar has more to select from does not mean that he must adopt another discipline and call it anthropology. The crux of the Boas tradition is the unity of humanity within its diversity and the search for that unity within the ever-changing kaleidoscope of human experience, as it is inherent in the mental life of man. Ours is a science of humanity – a scientific humanism with a multidisciplinary base.

2. Lewis Henry Morgan and Franz Boas: Continuity not conflict

> '... in his [Morgan's] view ... learning was not a quest for distinction or a search for ideology, but a voyage towards distant horizons of knowledge.' Carl Resek (1960, p. 163)

It is important to relate the work of Boas to that of Lewis Henry Morgan, who represents the flowering of what can be called the native American tradition in anthropology. Morgan was born in 1818, 40 years before Franz Boas and 42 years after the Declaration of Independence was signed. As a lawyer, he was a man of affairs, and the career of the democratic society proclaimed in the American Revolution was of direct concern to him. Boas was born ten years after the 1848 Revolution in Germany and identified himself as a professional scholar; he envisaged the realization of the ideals of the Revolution through learning not, however, as he stated more than once, through the academy. Morgan did legal work for the American railroad and mining interests, in which he was a successful financial investor. It is one of the ironies of history that Morgan became a major influence in the development of a new scientific ideology in the Soviet Union, whereas Boas became a major innovator and organizer of the educational structure of a newly crystallizing field of human science in America. Morgan carried his part in public affairs as his profession; Boas made it a personal crusade. Morgan pursued his scholarly work as a vocation, a personal preoccupation; Boas made it his profession. Morgan died on December 17, 1881, leaving his entire fortune of $80,000 for the higher education of women at the University of Rochester. Boas died on December 21, 1942, at a luncheon at the Columbia Faculty Club, where he was entertaining the French anthropologist Paul Rivet who was in exile from the Nazi occupation. As the meal was almost over, Boas spoke his last words: 'I have a new approach to the race problem.'

Boas' focus was on the intellect, which he conceived as the most vital faculty of man. He felt that the structured image we derive from the individual phenomenon should be under continual re-evaluation as we examine the data. Morgan's focus was on human society. As a lawyer and legislator, he put his emphasis on social constructs that should be established.

The United States of the early nineteenth century was an expanding society; Morgan's earlier vision was interrupted by the full onslaught of

the Industrial Revolution and the rise of the middle class, which left little room for attention to radical revision of the political structure. In Germany, the supernatural doctrine of aristocracy was replaced by racism, a biological explanation of the loser in the struggle for power. Bismarck was encouranging the rise of industry as a reaction to England's industrial advance, and colonialism, the use of other people's bodies and lands to feed raw materials to the machines, was moving apace. Internal exploitation within Germany was now supplemented by external exploitation. Boas found himself in the midst of this struggle and, when he came to the United States, in an even more immediate one against racism, which was undermining the expressed democratic principles on which the country had been founded.

Since the comparison of the American experience to the Golden Age of Greece was common in the United States, Morgan, an advocate of natural law philosophy, was disturbed that the early Greeks had been ruled by an aristocracy. The panic of 1837 made it difficult for him to obtain a position, and he took up the management of the family farm while pursuing his studies of Greek history. Morgan set out to prove that democracy was rooted in the laws of nature and that was the task he set for himself in his ethnographic studies. He had, in the first instance, come to his concern with the Iroquois while still a boy digging for arrowheads in upper New York state and, at college, had set up a Greek letter fraternity, later converting it to an Iroquois model. At one time he stated: 'It would have been a bright example to friends of democracy if some people, aided by the simple impressions of nature, had erected a republic under which to spend their infancy as well as maturity' (Resek, 1960. p. 17).

In the theology of the day, the savage was an example of the fall from grace. Morgan denied this doctrine, maintaining that civilization was removed from savagery only by time and learning. Morgan was not altogether pleased with this civilizing process. As against the Greeks and their urbane abstractions, he was coming to admire the aborigines' freedom, sincerity and independence. Indeed, the Seneca scholar Eli Parker, whom Morgan encountered in the former's youth, introduced him to some of the leading Iroquois, and this led to a lifelong interest, first expressed in the *League of the Ho-de-no-sau-nee*, which attracted international attention.

Morgan appears to have been the hub around which pre-Boasian American anthropology formed. His home was a clearing house for

proposed and past research in ethnology, and he maintained a large correspondence with scholars engaged in archeological and anthropological study: John Wesley Powell, Henry Gillman, John S. Clark, Otis T. Mason, Jeffries Wyman and Frederick W. Putnam. He successfully promoted archeological excavations by Bandelier, a close friend and student, in the Southwest and in Yucatan. He was the mediator between American anthropology and European ideas, traveling in Europe–meeting Darwin, McLennan, Tylor and Lubbock–and carrying on a correspondence with such people as Sir Henry Sumner Maine, who had become interested in Morgan's researches in the light of his own studies of ancient law.

As a lawyer, Morgan became involved in efforts to rescue Iroquois land from appropriation by unscrupulous land companies, and in 1862 he sent a series of proposals to President Lincoln for the revision of Indian policy and for the reform of the Indian Department. Morgan gave many public lectures in order to inform the people, maintaining that inequality among men was social and artificial not innate. He pictured an ideal society as homogeneous and free of all permanent divisions according to religion, education, ownership of property or advantages of commercial enterprise. During this time he was formulating what finally matured in *Ancient Society:* Man's only hope for survival is the realization of his egalitarian nature. And in his *Houses and House Life of the American Aborigines*, he opened the path to the most advance of our social concerns – the quality of our life in the most intimate social sense.

Morgan links this concern to the growth of private property, the legitimating ideology of which he attacked in *Ancient Society:*

> Since the advent of civilization, the outgrowth of property has been so immense, its forms so diversified, its uses so expanding and its management so intelligent in the interests of its owners, that it has become, on the part of the people, an unmanageable power. ... A mere property career is not the final destiny of mankind, if progress is to be the law of the future as it has been of the past. The time which has passed away since civilization began is but a fragment of the ages yet to come. The dissolution of society bids fair to become the termination of a career of which property is the end and aim; because such a career contains the elements of self-destruction. Democracy in government, brotherhood in society, equality in rights and privileges, and universal education foreshadow the next higher plane of society to which experience, intelligence, and knowledge are steadily tending. It will be a revival, in a higher form, of the liberty, equality and fraternity of the ancient gentes.

Morgan had brought 'historic insight into the relations of property to modern civilization', observed a reviewer in *Frank Leslie's Illustrated Newspaper* in England; but for this, 'the capitalist and proprietor may be tempted to suppose that the existing forms of property are invested with a certain divine right' without regard to the social labor which produced them or the social ends which they serve. An American interpretation, concurred in by Morgan, came from John Wesley Powell (Resek, 1960, p. 143), who thought that Morgan had shown that monarchy and feudalism were 'pathological conditions of the body politic – diseases which must be destroyed or they will destroy – and hence disappearing by virtue of the survival of the fittest'.

'Each level of advancement', wrote Morgan, 'marked an ethnic period during which ideas of government, of the family, and of property assumed distinct characteristics'. Government thus followed two plans,

> ... using the word *plan* in its scientific sense. Both were definite and systematic organisations of society. The first and most ancient as a *social organization,* founded upon gentes, phratries, and tribes. The second and latest in time was a *political organization,* founded upon territory and upon property. Under the first a gentile society was created, in which the government dealt with persons through their relations to a gens and tribe. These relations were purely personal. Under the second, a political society was instituted, in which the government dealt with persons through their relations to territory, e.g., the township, the county, and the state. These relations were purely territorial. ... Henry Maine had put the case similarly in his *Ancient Law.* But Morgan's application was revolutionary. Using the Iroquois as the classic example of 'social organization', their gentes, phratries and tribes were shown to be identical with the gentes, phratries and tribes of Greece, with the curia, tribe and populus of Rome. Fison's Kamilaroi, Bandelier's Aztecs, the Scottish clan, the Irish *sept,* the *thums* of India, and the twelve tribes of the Jews all illustrated a form of society through which man must pass on his way to civilization (Resek, 1960, 138–139).

'Hope for the future of society is the best loved daughter of evolution', concluded Powell. Morgan commented to Powell on his conception of the survival of the fittest: 'It is a tremendous thrust at privileged classes, who have always been a greater burden than society could afford to bear' (Resek, 1960, p. 143).

On his first trip to England, Morgan had been '... pained by the beauty of the English countryside because the land belonged to the few and not to the farmers who tilled it.... He saw few signs of progress.... The middle classes, far from destroying the aristocracy, imitated

its rule and its tastes ... in London he watched a workingmen's demonstration against a proposed parliamentary pension to Prince Arthur. They carry a mountain on their shoulders, he thought, but they are quite certain to persevere' (Resek, 1960, pp. 122, 124).

Boas' struggle against inequality took the form of an attack on racism in terms of demographic biology and a broad program of public lectures. For example, in 1910 he had given a talk at the Second National Negro Conference entitled 'The real race problem' (Boas, 1910). Here, all the statistical points were presented – man as a domesticated biological form, the overlapping of variations and other familiar arguments. He also discussed the handicap of slavery, the suppression of any kind of group identity and its implications. The other aspect of his teaching program had to do with man's need for beauty and for new insights into his own psychic nature. The subjective was not a problem for Morgan who, as an empiricist, did not recognize its significance. Boas, on the other hand, recognized that stimuli were received differently according to the mental state of the subject, quality contending with quantity so as to blur the clear distinctions of simple observation.

Franz Boas and Lewis Henry Morgan considered the human being on two different levels: Morgan investigated the evolution of social structures, as these would hopefully eventuate in a future classless and egalitarian society; it was to this end that he became interested in the commune as an intimate social form. Boas sought the expansion of the human imagination through a greater understanding of the aesthetic and its possible organic basis. There is no question but that he shared Freud's view of the artist:

> I should like to direct ... attention for a moment to a side of phantasy-life of very general interest. There is, in fact, a path from phantasy back again to reality, and that is–art. ... The way back to reality is found by the artist thus: He is not the only one who has a life of phantasy; the intermediate world of phantasy is sanctioned by general human consent, and every hungry soul looks to it for comfort and consolation. But to those who are not artists the gratification that can be drawn from the springs of phantasy is very limited; their inexorable repressions prevent the enjoyment of all but the meager daydreams which can become conscious. A true artist has more at his disposal. First of all he understands how to elaborate his daydreams, so that they lose that personal note which grates upon strange ears and become enjoyable to others. He knows how to modify them sufficiently so that their origin in prohibited sources is not easily detected. Further he possesses the mysterious ability to mould his particular material until it expresses the ideas of his phantasy faithfully; and then he

> knows to attach to this reflection of his phantasy-life so strong a stream of pleasure that for a time at least the repressions are out-balanced and dispelled by it. When he can do all this, he opens out to others the way back to comfort and consolation of their own unconscious sources of pleasure, and so reaps their gratitude and admiration ... (Freud, 1920).

In the deepest sense, then, the work of Franz Boas and Lewis Henry Morgan complement each other–Morgan is the prophetic analyst of social evolution; Boas deploys science as the arbiter of the dignity and potential of the human person. Together, they constitute both an intellectual tradition and a political perspective whose pertinence remains undiminished.

References

Benedict, Ruth (1934), *Patterns of culture.* Boston, Houghton, Mifflin.

Boas, Franz (1940), *Race, language, and culture.* New York, Macmillan.

— (1938), *General anthropology.* New York, D. C. Heath.

— (1911), *The mind of primitive man. ...* A course of lectures. New York, Macmillan. 1938[2] (revised).

— (1910), The real race problem. *The crisis.* National Association for the Advancement of Colored People. Pp. 22–25. Reprinted as *The real race problem from the point of view of anthropology.* Publication, No. 3, National Association for the Advancement of Colored People, 1910.

— (1908), *Anthropology.* New York, Columbia University Press.

Freud, Sigmund (1920), *A general introduction to psychoanalysis.* New York, Boni & Liveright.

Kluckhohn, Clyde, and Prufer, Olaf (1959), Influences during the formative years. In Walter Goldschmidt (Ed.), *The anthropology of Franz Boas.* Memoir, No. 89, American Anthropological Association, October.

Kroeber, Alfred, Ed. (1943), *Franz Boas, 1858–1942.* Memoir, No. 61, American Anthropological Association, July–September.

Laguna, Grace A. de (1966), *On existence and the human world.* New Haven, Yale University Press.

Morgan, Lewis Henry (1851), *League of the Ho-de-mo-sau-nee, or Iroquois.* Rochester, N. Y., Rochester Historical Society.

— (1871), *Systems of consanguinity and affinity of the human family.* Columbia, Smithsonian Institution.

— (1877), *Ancient society, or Researches in the lines of human progress from savagery to civilization.* New York–London.

— (1881), *Houses and house-life of the American aborigines.* Washington. [Geographical and geological survey of the Rocky Moutain region.]

Resek, Carl (1960), *Lewis Henry Morgan: American scholar.* Chicago, University of Chicago Press.

Tanner, J. M. (1959), Boas' contribution to knowledge of human growth and form. In Walter Goldschmidt (Ed.), *The anthropology of Franz Boas.* Memoir, No. 89, American Anthropological Association, October.

Weltfish, Gene (1965), *The lost universe.* New York, Basic Books. (Bison Books, University of Nebraska Press, Lincoln, 1977).

— (1962), Some main trends in anthropology in 1961, *The Annals of the American Academy of Political and Social Science*, Philadelphia, pa., 339 (january): 171—176.

— (1958), The linguistic study of material culture, *International Journal of American Linguistics,* 24 (October): 301–311.

— (1953), *The origins of art.* Indianapolis, Bobbs Merrill.

— (1936), The vision story of Fox Boy, a South Bend Pawnee text, *International Journal of American Linguistics,* 9 (1): 44–75.

White, Leslie A., Ed. (1937), *Extracts from the European travel journal of Lewis H. Morgan.* Rochester, N. Y., Rochester Historical Society.

— Ed. (1940), *Pioneers in American anthropology. The Bandelier–Morgan letters, 1873–1883,* 2 vols. Albuquerque, University of New Mexico Press.

7 Germany: Interpreting culture traits

PAUL LESER

I. Very early there was a rather striking divergence in European anthropology. In the second half of the eighteenth-century, when Friedrich Schiller (1789) in Germany expressed unilinear evolitionism in its most extreme form[1] and Adam Ferguson (and others), prior to Schiller, had formulated somewhat similar ideas in Schotland (Slotkin 1965, p. XV and 436, no 3, generally 430–443; Voget 1965; Voget 1967, pp. 132 f., 135, 142 f., 148; Voget 1968 pp. 321–323, 325 f., 330, 333–339, 341), Anders Berch (1759, 1773, 1953) in Sweden initiated several of the methods which later, accepted by numerous scholars, came to prevail in ethnological research in many European countries (Erixon, 1938, p. 247; 1948, p. 129; 1962–1963, p. 98f; 1964–1965, p. 16; Kothe, 1957–1958, p. 61; Berg, 1961, p. 18; Bringéus, 1962–1963, p. 32).

1. as stressed by Graebner who, more than sixty years ago, emphasized Schiller's impact on the development of anthropology. In spite of the repeated references by Graebner (1906, p. 181; 1911a, p. 78; 1911b, pp. 85, 87; 1923, p. 437; 1927, p. X), most studies on the history of anthropology (e.g. Haddon, 1934; Lowie, 1937; Mühlmann, 1938 and 1948; Numelin, 1947; Penniman, 1952; Hays, 1958; Bianchi, 1965; Mercier 1966; Harris, 1968; Poirier 1969; Broce 1973) do not even mention Schiller, an exception being Slotkin who published excerpts from Schiller's inaugural lecture based on C. J. Hempel's translation of 1861 (Slotkin, 1965, pp. 384–385). Historians, documenting their renewed interest in Schiller, recently published a new translation (*History and Theory*, vol. 11, 1972, pp. 321–334). I cannot go along with either translation. Schiller's *Stufen der Bildung* translated as 'degrees of culture' in Slotkin, and as 'levels of development' in *History and Theory*, in my opinion should be translated as 'stages of evolution'. Because of Schiller's importance for the study of the history of anthropology, a more satisfactory translation at least of the key passage (paragraphs 10–12 in any German edition) is needed. Therefore, in Appendix 1, some words from the two translations are listed, along with Schiller's text and the wording which I suggest.

First of all, Berch is one of the earliest exponents of *distribution studies*. His claim 'that it should be possible to arrive at certain conclusions about the origin and migrations of' peoples (Jirlow, 1953, p. 100) by examining the distribution of the different forms of culture traits anticipated Ratzel's and Graebner's main methodological tenets. By stressing the importance of the geographical boundaries of objects he led the way to *cartography*. Second, he stressed the study of *European* (not narrowly national) *ethnology* (*vergleichende Volkskunde*, not Swedish nor German nor any other '*Volkskunde*'). Third, he emphasized the *study of material culture* and in particular of agriculture and agricultural implements, thus creating a trend which has produced a multitude of investigations nearly everywhere in Europe. Fourth, by insisting on the need for comparisons he took a stand against narrowly *local interpretation* as well as against unwarranted *teleinterpretation*, thus introducing a trend which became dominant through Ratzel and Graebner. (Berch's contemporaries, it seems, were more impressed by his numerous other contributions. Ferrner, 1776, mentions none of Berch's views referred to above nor the articles in which they were presented.)

II. I will say nothing about the *study of material culture* because it is well known that to this day it survives on the Continent while in Great Britain and the United States research dealing with material culture has been on the decline.[2] In discussing distribution studies, cartography and regional European ethnology, I can be brief. While today, in Scandinavia and in most other European countries, regional European ethnology is stronger and more important than ever (Fenton, 1973), it does not exist in the United States as a separate academic discipline (Leser, 1967). Many American anthropologists seem to be unaware of the work being done in that field by European scholars or at least seem to con-

2. Leser, 1952, p. 537; 1960a, p. 257. It must be said, however, that lately the study of material culture has declined also on the Continent. In 1973 Steensberg (p. 9) claimed quite generally, without confining his statement to Great Britain and the United States, that modern anthropologists do not sacrifice much time in studying the material culture of everyday life, being more interested in other research. Doubts have been expressed even concerning the usefulness of the very term "material culture", see Wiegelmann, 1970, and Leser, 1972a. On the other hand, in the United States a resurgence of an interest in material culture can be notices, see e.g., Ellsworth and O'Berien (1969) and in particular, recent trends in, and publications by, the American Folklore Society.

sider it no part of anthropology,[3] and therefore to be ignored. At the 66th Annual Meeting of the American Anthropological Association in Washington, D. C., for example, in a paper on 'Ethnology in Yugoslavia' by Joel M. Halpern and Eugene A. Hammel (see Halpern and Hammel, 1967a, 1967b, 1969; Halpern, 1970–1971), no reference was made to the work of Branimir Bratanić and Milovan Gavazzi nor even to the Ethnological Atlas of Yugoslavia. The influence of Yugoslavian ethnology on Czechoslovakian, Hungarian, Austrian, German, Scandinavian, Portuguese ethnology likewise was not mentioned.

To some extent the lack of communication is mutual. American folk culture may be considered an outgrowth of European folk cultures and its study closely related to regional European ethnology. Yet, in many instances, American folklife students probably consider themselves to be historians or literary historians rather than anthropologists.[4] Therefore they have not felt the need to become members or fellows of the American Anthropological Association (see Frantz, 1967) or Associates in *Current Anthropology* or even 'Associates of Associates' (*Current Anthropology*, 1967, 8 [5]: 549), or, to use the wording of the Third Edition of the *International Directory of Anthropologists* (Herskovits and Ames, 1950, p. 205), 'persons known or believed to be active in anthropology' (Fourth International Directory, passim).

As to *cartography*, I feel it is only fair to say that although maps to this day have been compiled by American linguists (e.g., Kurath and McDavid, 1961) and at times even by American cultural anthropologists e.g. Kroeber 1925, 1941, 1947; Roberts 1936; Riesenfeld 1950; Simoons 1954, 1961; Driver and Massey 1957; Kramer 1966. The compiling of maps for *distribution studies* has remained predominantly a European practice.[5] It was Ratzel who emphasized the necessity of map work

3. However, as an indication of a beginning interest among American anthropologists in regional European ethnology, Theodoratus, 1969, and Burcaw, 1973, may be mentioned. Also, 'an informal group of Italianist scholars' has been 'launched' which, includes scholars working in 'human biology, prehistoric and classical archeology [and] linguistics' (Moss, 1974, p. 10).

4. Leser 1970–1971, p. 278. Gerschenkron, 1971, p. 351, considers anthropology and *'Volkskunde'* to be synonymous. Jensen, 1960b, p. 1, on the contrary, does not deem European ethnology a part of general ethnology.

5. The maps by Kroeber, Roberts, Rieserfeld, and Riesenfeld, Simoons, Driver and Massey, and Kramer just mentioned, as well as the maps to which this paragraph and the following refer, were compiled for research purposes; they are an heuristic device. Maps compiled as teaching tools, e.g., Turney-High, 1949, p. 189, Spencer, 1956, Spencer and Johnson, 1960, are quite another thing and therefore not

(1891, pp. 730–781, particularly 761–769; in the editions of 1912 and 1922, pp. 474–509, particularly 495–499; see also p. 552); it was Ratzel to whom the break-through is due and who has been followed by an entire school, composed of scholars who had studied under him as well as by scholars who had not been his students. Frobenius' work (1897, 1898a, 1898b, 1900; *cf.* 1907, p. 313) which started with maps may be said to have culminated in the *Atlas Africanus* (Frobenius 1922–1930, 1931, 1932) and in the *Kulturgeschichte Afrikas* in which he praised the cartographic method as a 'sicheres Instrumentarium' (1933, p. 80). Graebner constantly drew and made use of maps (1905, pp. 30, 31, 41, 45, 46, 51; 1906, p. 183; 1909b, pp. 731, 752, 999; 1923, pp. 518, 519) (although he theoretically belittled the cartographic method as a mere 'technical device, not an integral, let alone decisive, part of the method'; 1911, p. 133, n. 1). Schurtz (1889, 1902, p. VI), Weule (1899), Adler (1901, 1902), Ankermann (1901, 1905b, pp. 56, 58, 62, 65, 69), Johannes Lehmann (1904), Pessler (1906, 1910, 1911, 1912 and many other publications) (all of whom were Ratzel's students[6]) continued the tradition of their teacher. They, in turn, were followed by their students (e.g. Plischke, Kroll, and Baumann)[7] as well as by others (to mention just two: Struck and Vatter).[8] The next generation (e.g. Grau and Drost)[9] again carried on, and the compilation of maps for ethnological research has remained a German and Swiss technique to this very day.[10]

---

to be considered here. Simoons and Kramer (both coming from the school of Carl Sauer) are professors of geography; on the basis of the work which they have done I have taken the liberty to claim them as 'American cultural anthropologists'. It is not insignificant that anthropological research which I consider to be of particular importance has been done by three men who officially are geographers (the third one being Erich Isaac (see below p. 0000). In 1973 Eric P. Hamp put in a plug for atlases stating that 'vastly more cross-cultural mapping can and must be done' (1973, p. 333). Even more recently, in 1974, an American map compiled for reserach purposes, has been republished: Eames and Robboy, 1974, p. 507.

6. On Frobenius' relations to Ratzel, see Frobenius 1925, pp. 50, 55–59.

7. Plischke 1922, map following plate 2; Kroll 1928, pp. 188, 196, 207, 215, 224, 271; Baumann 1928, pp. 293, 303; 1944a, pp. 208, 215, 223, 226, 229, 256, 258, 266, 295; 1948, pp. 24, 25, 37, 41, 49, 56, 66, 77, 83, 88–89, 113–114, 159–160, 173–174, 203–204, 217–218, 247–248, 261–262, 275–276, 289–290, 319–320, 349–350, 363–364, 393–394, 439, 440; 1955, four maps between pp. 352 and 353, p. 361.

8. Struck 1909a, p. 87; 1909b, p. 175; 1922, pp. 51–113. – Vatter 1926, pp. 566–579.

9. Grau 1955, p. 170. – Drost 1967, pp. 19, 33, 55, 63, 69, 79, 113, 119, 139, 141, 159, 167, 175, 185, 197, 217, 219, 1968, p. 138.

10. e.g. Kauffmann, 1968, p. 310. – Marschall, 1968, maps 1, 2, 3, following pp. 32, 188, 192. – Tiesler 1970, pp. 196, 204. – Seiler–Baldinger 1971, twelve maps between pages 250 and 251.

Similarly, outside of Ratzel's immediate sphere of influence, cartography has remained a tool continuously used in general ethnology, particularly, but not exclusively[11] in Sweden. Many of Nordenskiöld's (e.g., 1924 [1919–1938]) and Gerhard Lindblom's[12] publications would be inconceivable without distribution maps as would those of the entire 'Swedish school'[13] as it has been called.[14]

Yet cartography's triumph came mainly in regional European ethnology (to single out one striking example, Weiss, 1962, pp. 200–231). For the past hundred years, many of the successes of regional European ethnology must be traced to the compilation of maps.[15] In regional European ethnology, the emphasis on cartography, today more prolific than ever,[16] again is directly due to Ratzel (Pessler, 1906, pp. IX, X;

11. See, e.g., Róheim 1926: maps following pp. 56, 68, 92, 106, 162, 166, 308, 338, 342, 364, 410, and maps on pp. 399 and 405.

12. A few random examples: Lindblom, 1927a, p. 24; 1927b, p. 12; 1928, p. 23; 1933, pp. 19, 47; 1939, p. 35; 1949, fig. 16.

13. Again, only a few random examples: Lagercrantz, 1935, pp. 148, 149, 150; 1937, pp. 420, 426, 429, 430, 432, 433, 434, 435; 1938a, passim; 1938b, p. 205; 1939a, pp. 39, 41, 43; 1939b, p. 373; 1950, passim; 1954, pp. 61–75; 1968, p. 126; 1973, p. 571; Söderberg, 1956, p. 143; Widstrand, 1958, p. 165ff.; Anell and Lagercrantz, 1958, pp. 15, 17, 19, 65, 68, 69, 71, 73, 77; Anell 1969, pp. 119–126.

14. Lagercrantz, 1954, p. 1. – The Swedish school has been ignored (should I say conveniently?) by several writers of histories of anthropology. Eggan, 1968, Harris, 1968, Broce, 1973, say not one word about it. Mühlmann, 1948, p. 232f., Penniman, 1952, p. 331 and Hays, 1958, p. 292, do mention Nordenskiöld, but none of the others. Penniman lists Nordenskiöld's *Comparative ethnological studies* among 'more or less general works, mainly in the evolutionary school'! Hays gives a wrong date for Nordenskiöld's death. Sternberg (1926, p. 237), on the other hand, in the sentences preceding his mention of Nordenskiöld, says: *'Als äusseres Kennzeichen dieser Tendenz dient, dass keine mehr oder weniger ernsthafte Arbeit ohne Kartographierung ... auskommt.'* (Italics mine. P. L.)

15. Erixon, 1937 (basic); 1938, pp. 246–263; Weiss, 1951, pp. 96–110; 1952, pp. 363–373; Beitl, 1955, pp. 34–38; Jansson, 1961, pp. 109–114; Barabás, 1963; Svensson, 1966, p. 60ff.; Bericht, 1968, pp. 5–6; Rohan-Csermak (1966, p. 73) speaks of 'a blossoming of cartographic works as never before' ('un épanouissement jamais vu des travaux cartographiques dans notre discipline').

16. First to be cited is the ongoing work on the Ethnographic atlas of Europe (see, e.g., Fellenberg, 1970 and 1972) as well as the ethnographic atlases of individual parts of Europe already published (see particularly Erixon, 1960–1961, and Bratanić, 1972; for examples see Jaberg and Jud, 1943; Weiss, 1950; Geiger and Weiss, 1950; Erixon, 1957; Zender, 1958ff.; Burgstaller and Helbok, 1959; Wolfram, 1959; *Etnološki atlas,* 1963). see also Teesch 1961 a–f.

Individual studies using the cartographic method are so numerous that it is impossible to name even a representative sample of the most recent works. The following is merely a chance list: Ränk, 1966; Vilkuna, 1967; Granlund, 1968; Sandred, 1968; Zendes 1968; Hvarfner, 1968–1969; Jean-Brunhes Delamarre, 1969; Sarmela,

1932, p. 739); the emphasis, but by no means the beginnings. It was in 1876 that Strindberg, the dramatist, novelist, poet began working on his map showing the distribution of the various Swedish terms for the lady-bug (Strindberg, 1882, pp. 15–19; Nettelbladt, 1936, pp. 326 f. 331). He was not (and could not have been) influenced by Ratzel (Nettelbladt, 1936, p. 322ff.).

In some instances the spectacular results of the cartographic method in regional European ethnology are due in no small measure to the fact that *the study of the distribution of traits* added another source to the standard historical sources (such as 'objects' and 'texts').[17] According to Jones (1952, p. 993), historical reconstruction based on distribution studies and cartography may be 'useful' in European ethnology 'where it can fall back on corroborative archaeological and historical research' but not in general ethnology (where in the vast majority of cases such corroboration is not available). Both claims are wrong;[18] historical reconstruction, based on distribution studies, has been of importance in European ethnology precisely in those cases which lack corroboration, thus posing questions which otherwise never would have been raised: See, for example, the problem of certain relations between Asia Minor, the Balkan peninsula and Scandinavia pointed out by Bratanić (1960, pp. 221–228; see also 1951, pp. 221–250; 1955, pp. 90–98; 1961, pp. 63–65. – Leser, 1961); the problem of the plow missing in a large area of eastern Sweden (Jirlow, 1970, pp. 79–83); the problem of an otherwise undetected historical relation between Sweden and certain areas in the Alps (Rhamm, 1908, p. 1048; Hahn, 1909, p. 578; Leser, 1931, p. 328).

1969; Vilkuna, 1969; Jirlow, 1970; O'Danachair, 1970; Berg, 1971: van den Bergh, 1971; Bringéus, 1971; Cox 1971b; Fenton, 1971: Gavazzi, 1971; Goossens, 1971; Granlund, 1971; Kłodnicki, 1971; Nordland, 1971; Norsander, 1971; Paladi-Kovacs, 1971; Terent'eva, 1971; Valeri, 1971; Viires, 1971; Vuorela, 1971; Zender, 1971; Balassa, 1972; Balogh, 1972; Barabas, 1972; Bentzien, 1972; Besch, 1972; Bringéus, 1972; Burgstaller, 1972; Cox, 1972a, 1972b; Demtschenko, 1972; Dietz, 1972; Frauenknecht, 1972; Füzes, 1972; Grober-Glück, 1972; Hanisch, 1972; Karlinger, 1972; Kisban, 1972; Koren, 1972; Leiner, 1972; Moltschanowa, 1972; Neu, 1972; Rasmussen, 1972; Riemann, 1972; Šach 1972; Schindler, 1972; Takacs, 1972; Vakarelski, 1972a, 1972c; Vařeka, 1972; Viires, 1972; Vilkuna 1972; Zender, 1972; Zender, 1972; Rasmussen, 1974; Karasek and Lanz 1974. Of older publications special mention should be made of Peesch, 1957, see Leser 1970–1971, p. 292.

17. See Leser, 1970, p. 18. There is no generally accepted classification of sources.

18. For a general refutation of Jones see Lagercrantz, 1954, p. 1.

III. The common denominator of distribution studies in general, of regional Europan ethnology, of the emphasis on cartography and of the study of material culture is the interest in a genetic classification of cultures (see below section IX). Tenets generally held are (a) that without knowing the distribution a genetic classification of cultures is not possible (Bratanić, 1958, p. 1043; Höltker, 1968, p. 16), (b) that without genetic classification of cultures a correct interpretation of individual traits cannot be arrived at and (c) that therefore distribution is as important to the anthropologist as chronology to the historian.[19]

Although 'the classification of languages is not within the province of descriptive linguistics, ... many descriptive linguists also work on this problem' (Gleason, 1955, p. 333). Anthropologists are less tolerant. Most British and American (and, more recently, a growing number of Continental and Scandinavian) anthropologists not only refuse to work on problems of the geneitc classification of cultures but condemn any attempt of others to do so,[20] although cultures as well as languages 'are so numerous that no one can control this large mass of material without some sort of classification' (Gleason, 1955, p. 333). 'A typological classification may be useful for certain purposes' (Gleason, 1955, p. 345), but a sound basis for interpretation can be provided only by a genetic classification (Bratanić, 1958, pp. 1043, 1044). Thus, if faced with problems of interpretation, those anthropologists who reject all attempts to arrive at a genetic classification of cultures are forced to (or are casually willing) to base the interpretation of a phenomenon on the narrow local area from which they happen to know it (local interpretation, sometimes folk interpretation) or even simply on ideas which they ascribe

19. Kroeber: 'To an anthropologist a cultural phenomenon unaccompanied by its "distribution" ... is as nonsignificant a fact as an unplaced and undated event would be to a historian' (1948, p. 542). Kroeber then (1948, p. 542, note 1) quotes Lowie's dictum: 'When we do not know the distribution of a phenomenon, we know nothing that is theoretically singificant.' Kroeber does not say where this Lowie quotation may be found, nor does Bock (1967, p. 216) who quotes Lowie as follows: 'If you do not know the distribution of a trait, you do not know anything about it.'

20. The British and American hostility to problems of a genetic classification of cultures, to a considerable extent, is due to the (in this instance: disastrous) influence of Malinowski and Radcliffe-Brown (see, e.g., Malinowski, 1934, p. XVIII, and Radcliffe-Brown, 1953, pp. 3, 7, 58f., 86) who made it possible for later generations to enjoy, with a clear conscience (and even with the happy feeling of superiority) boy-scoutism and the creating of lofty theories instead of doing some (much needed, but) tedious work. (A sad case of the 'generational autonomy' and 'the way legitimate problems go out of fashion' deplored by Cora Du Bois, 1963, p. 35.)

to the culture in question (guessing teleinterpretation). (See below, Appendix 2.)

The trend of rejecting both local interpretation if it disregards the total geographical distribution and teleinterpretation if it cannot be justified geographically and/or historically, in my opinion, goes back to Anders Berch. Ratzel and Graebner, and no doubt, strongly affected the German, Austrian, Swiss and Scandinavian development of anthropology. Yet, it seems probable that the central and northern European attitude toward folk, local and teleinterpretation was greatly influenced by the work of linguists which in the nineteenth-century was both successful and impressive. The rejection by linguists (e.g., Greenough and Kittredge, 1968, pp. 144–154; Müller, 1877, p. 262f.) of folk etymologies and of etymologies based on one narrow dialect not representative of the entire language, to say nothing of the language group or language family, has strengthened the Berch–Ratzel–Graebner rejection of folk and local interpretation while the successful establishstet of language families such as Indo European led many anthropologists to follow Ratzel and Graebner in attempting to find the original meaning of a phenomenon in the culture in which it originated.

It seems strange that there is no international consensus among anthropologists with regard to folk, local and teleinterpretation. Linguists, irrespective of their national background, are in agreement in their view of folk etymology; every European linguist, I assume, would concur with Hockett and Sturtevant. Hockett (1968, p. 288) states: 'A folk-etymology is an invented explanation of why a certain form means what it does, and the invention, no matter how far-fetched, usually turns somehow on the same sort of vague similarity of shape which underlies metanalysis and reshaping.' Sturtevant (1947, p. 117ff.) uses the term 'popular etymology' 'which seems to contrast with scholarly etymology, scientific etymology, or the like' (p. 118) but which simply 'applies to naive interpretation' (p. 119). His terminology thus comes very close to Max Müller's 'fanciful' etymology (1877, p. 267) and 'guessing' etymology (1877, page titles on pp. 263–267). The example of Hockett (1968, p. 288) is to the point: 'A student asked: "Are *affricates* called that because they are extremely common in the languages of *Africa*?"'

Folk etymologies are 'invented explanations' (Hockett) based on insufficient comparative material or too narrow an experience. The same holds true for folk interpretations (Raglan, 1949, p. 31). Therefore,

they must be repudiated just as definitely (although, of course, they should be diligently collected and carefully recorded). Occasionally (as in the first five of the following examples) it is not even necessary to write a comment in order to discredit them:

In Australia, in the vicinity of Port Jackson and the Botany Bay, 'women are early subjected to an uncommon mutilation of the two first joints of the little finger of the left hand' (Söderström, 1939, p. 36, quoting Collins, 1804, p. 358). The two first joints are severed, according to the folk interpretation, in order to enable the women better to 'wind their fisching-lines over the hand' (Söderström, 1938, pp. 36 and 34.)

'The Dieri say... that the object of the kulpi operation [subincision] is "cleanliness".' (Stuart, 1896, p. 119.)

In the nineteenth-century a folk interpretation of circumcision gained wide circulation (Gray, 1911, p. 659); circumcision was explained as a measure to prevent uncleanliness.

Nineteen hundred years earlier Philo, writing about circumcision, listed among 'the real causes of the ordinance' and 'the reasons why the custom has prevailed', 'first of all that it is a preventive of ... carbuncle' and 'most important' that after circumcision 'the seminal fluid proceeds in its path easily, neither being at all scattered, nor flowing on its passage into what may be called the bags of the prepuce', thus making 'those nations which practise circumcision ... the most prolific and the most populous' (Philo, 1855, p. 173f; 1906, vol. 5, p. 122).

In some villages in the Bogadjim area of New Guinea, the explanation given for circumcision is that it will assure the future marriage in resulting in good looking and graceful children only (Thurnwald, 1930, p. 382).

In 1957 F. R. Lehmann published a highly important and illuminating article 'Towards a new explanation of circumcision' which is based mainly on the folk interpretation of the Bambara. It is, of course, possible that among the Bambara the true reason for circumcision remained known from times immemorial on. Besides their explanation, Lehmann mentions others, most of which are simply guessing tele-interpretations. When Lehmann calls his article 'nothing but a preliminary' treatise because it is not supported by 'a discussion of the possibility to map the distribution of circumcision' (p. 57), it is evident how strong the influence of Berch, Ratzel and Graebner has remained.

If we are to accept the Bambara interpretation of circumcision as being correct, then we must assume that the meaning of circumcision was passed down from the day of the first introduction to the day when

the Bambara told it to Europeans. Such conservatism cannot be ruled out. Yet, it must be said that he who deems folk interpretations in general to be trustworthy, assuming that the original meaning survived without change from generation to generation, believes in a 'static conception of culture' (see below, p. 000).

Frequently, folk etymologies and folk interpretations are presented simultaneously: In Algeria a fanning mill is used by the Arab population (for an illustration, see *Anthropos*, 63, plate 2, following p. 208). It is not used by the Kabyles, although Kabyles, at least prior to the independence of Algeria, occasionally borrowed fanning mills which were in the possession of French institutions. This implement is called *tarare*. Arabs claim (a) that it is an Arab invention (folk interpretation!); (b) that its name derives from Arabic *dhara* (folk etymology); and (c) that the French took the implement itself and the word *tarare* from them (folk interpretation and folk etymology). On the surface these claims seem rather probable, for three reasons: (1) The Arabic word *dhara* means 'to winnow'; (2) in French, the word *tarare* (in the meaning fanning mill as well as in other meanings) is unexplained etymologically; in the meaning fanning mill it is quite recent, not documented before 1874 (Steiger, 1956, p. 205); (3) in no other Romance language, not even in Provençal, does a word similar to *tarare* appear as the name for the fanning mill (Steiger 1956, p. 205). The fanning mill is known, e.g., in Italy as *macchina vande; vandadora; ventolatore;* etc. (Jaberg and Jud, 1943, pp. 140, 231, 248) and in Spain as *avantadora* (Steiger, 1956, p. 205).

Before French *tarare* acquired the meaning of fanning mill it was used (as it is to this day) as the name of a city in France (*Petit Larousse Illustré*, 1912, p. 1612), as the term for a type of cloth made in Tarare (Schwann, 1807, p. 1125) and as an interjection meaning approximately *fiddlesticks* (*Petit Larousse Illustré*, 1912, p. 970; *Dictionnaire complet*, 1883, p. 229).

Yet, the folk etymology and folk interpretation mentioned must be rejected. Although Arabic *dharra* means 'to winnow' and classical Arabic *midharra* means 'an instrument with which grain is scattered', *tarare* could be derived only from a word such as *dhar(r)ara*, and no such word exists in classical Arabic or in any of the dialects from Mesopotamia to Morocco (Steiger, 1956, p. 205). The folk etymology, therefore, is rejected by the Arabists (Steiger, 1956, p. 205).

The fanning mill itself definitely is not an Arab invention but derives from – China! There it was in use as early as the Han (if not earlier) (Needham, 1965, p. 118, plate CLVI). Theoretically, Arabs could have been the intermediaries between China and the West as they have been in so many other instances. A decision whether or not they were must be based, according to the teaching of Berch, Ratzel and Graebner, on the distribution of the implement. In Arab territory, the fanning mill and the word *tarare* can be documented only for Algeria, not for any area where there have been no French settlers. There is no indication that Arabs ever used the fanning mill prior to the twentieth-century. On the other hand in the Western world, the fanning mill, in every detail identical to the model used by Arabs in Algeria, is much more widely distributed than merely in France. In the nineteenth- and early twentieth-centuries it was also quite common in Spain, Italy, Switzerland, Germany and Hungary. For the eighteenth-century, it can be documented for Scandinavia, the Netherlands, England, Scotland (Leser, 1928, p. 449b; 1931, p. 454; Berg, 1928, pp. 19–24; Jirlow, 1936, 291–298), Pennsylvania (Yoder, 1965, p. 18) and New England.[21] This distribution would not be possible if the French had adopted the fanning mill only after their conquest of Algeria in 1830 and the machine had been unknown to European peoples before that time.

IV. Although Hans Manndorf (1952, p. 148) advocates a 'konsequent ... durchgeführte Lokalinterpretation',[22] it must be said that an interpretation must be rejected if it is based exclusively on a single, restricted area and fails to examine the entire distribution of the object, custom or concept in question (see also Barthel, 1961; Bringéus, 1962–1963, p. 35 and his quotation from Mannhardt). Local interpretation as stated by

21. Two fanning mills in the Hill-Stead Museum in Farmington, Connecticut. In 1973, the future of the Hill-Stead Museum was threatened (see Demeusy, 1973). This shocking portent was the more disturbing since the unique collection of New England fanning mills dating from 1780 to 1820 in Old Sturbridge Village was lost in the flood of 1955 – an irreparable loss.

22. An article by Koppers 'Krauses Strukturlehre als Teil der Lokalinterpretation. Würdigung und Kritik, *Anthropos*, 1927, S. 314' to which Manndorf refers (1952, pp. 140, 149) does not seem to exist. See Index, 1949, pp. 21–24; Rahmann, 1957, pp. 271–274; Anders, 1965, pp. 1–17 (in particular 1–9 and 13). In Koppers' article of 1927 on Krause, 'Fr. Krause's "Strukturlehre" als Teil der kulturhistorischen Methode', no mention is made of local interpretation. Page 314 in *Anthropos* 1927 is part of a review by R. Battaglia. The preceeding pages, 311–313, contain a review by Koppers but not about Krause.

Graebner (1911, p. 168) is permissible only if the 'local nature' of the trait to be explained has 'been unquestionably assured' (1911, p. 168; *cf.* p. 93). Interpretations (e.g. of myths) based on single local forms, always are of doubtful validity (Graebner, 1909c, p. 363). 'An explanation based upon a local cultural situation is admissible ... only in the case of those traits and forms which culture historically show themselves to be distinctly local features.'[23] In his rejection of methodologically incorrect local interpretation, Graebner (1911, p. 92) specifically refers to Ratzel's stimulus in this matter.

Koppers, criticizing Otto Reche, has provided us with an example for local interpretation (1928, p. 686) which I consider worth[24] quoting. According to Reche, the round hats and caps in the Alps owe their existence to the there-predominant brachycephaly, while silk hats go together with the dolichocephalic and long-faced population of North-Central and Northern Europe. Also, long pants are to be explained by belonging to the tall, slim body of Nordic man. Loose, long gowns, such as the caftan and the kimono, are due to the body-build of Asian peoples. Such local intepretation, as Koppers correctly emphasizes in his criticism of Reche's paper, presupposes that round caps and silk hats, long pants and loose gowns, are indigenous to the respective areas.

Frequently it becomes difficult to separate folk interpretation from local interpretation. An example of how to disprove both has been given by Leopold Schmidt (1957) in a brilliant paper: In a certain area of Carinthia, the method for sowing is carefully prescribed. I omit many of the details, mentioning only those which are pertinent to my argument. The farmer sows broadcast. A sow-cloth in which he carries the seed to be scattered is tied around his waist. From his sow-cloth he takes one handful of seed sufficient for three castings. He is assisted by his wife who walks opposite him where the castings end. She carries a heavy wooden trough (for illustrations, see Fahringer, 1971, and L. Schmidt, 1957, p. 786) filled with seed. Whenever the farmer's sow-

23. Graebner, 1911, p. 168. In the above wording I have made use of the translation (by Frida Ilmer, p. X) in Calverton, 1931, p. 426. Graebner's original German of the sentences quoted can be found in Appendix 3. In addition, see also Graebner 1908b, p. 1005: '...muss ... erneut darauf hingewiesen werden, dass es bei dem heutigen Stande unserer Wissenschaft nicht angeht, weit verbreitete Dinge an jeder Stelle der Erde mit Gewalt aus den örtlichen Verhältnissen erklären zu wollen.'

24. 'Krasse Fälle sind ja ... leichter zu beurteilen als Durchschnittsleistungen' (Kirn, 1963, p. 67).

cloth is empty, she replenishes his supply by filling his sow-cloth from her trough. When her trough is empty she goes to a large bag which is situated somewhere on the field, containing the main supply of seed, and fills her trough from that bag. She is not permitted to remain where the bag is standing nor may she rest anywhere else but must go on walking, watching her husband continuously. The farmer himself must not take the seed from the main container. In other parts of Carinthia (and in most areas of the world where sowing is done broadcast) the farmer himself refills his sow-cloth (or his sowing-apron, etc.).

The folk interpretation claims that this is done in order to prevent the farmer from having the sowings overlap one another and in order to prevent him from leaving any area unseeded. Since the wife walks along at the end of the castings, the explanation would seem to make sense. Yet it does not explain why the farmer himself is not permitted to approach the main seed container. A strictly local interpretation, and an interpretation as if these customs had come into existence today, would claim that the interposition of the wife increases the importance of women and is a symbol of the improved status of womanhood. The close participation of the wife in the work of her husband makes for stronger matrimonial bonds.

It would be impossible, by examining only the area in which the custom exists, to arrive at a valid interpretation. The folk interpretation itself, although admittedly of interest to the ethnographer, limits itself to the present-day custom, ignorant of its history and of the wider distribution of the components of the custom and the implements involved.

Only after plotting on maps the distribution of the sow-trough, the use of the sow-trough as measure and of sowing baskets (L. Schmidt, 1957, pp. 783, 793, 795), did Schmidt ascertain that the origin of the custom was in an entirely different culture – not in the culture of today which knows or even stresses the responsibility, self-reliance and independence of the farmer or peasant but in the mediaeval economy of servitude where a delegate of the lord apportioned the seed, measure for measure, to the peasant (L. Schmidt, 1957, pp. 773–802). The interposition of a measure between the supply bag and the sowing process itself made sense in an economy where the lord did not trust the sower and had him watched continuously while sowing, and where an accounting had to be rendered. In an economy in which the farmer is his own master, such supervision and accounting are unnecessary.

Since these are inexplicable and absurd, they lead to a folk interpretation which misconstrues.

Only by devising maps and through culture-historical research was it possible to trace the custom to its origin and to explain to what it owes its existence – not necessarily its survival – to this day.[25] The anthropologist should not accept an interpretation which does not explain the original meaning – no more than would the linguist. Where it is impossible to arrive at the original meaning, no valid interpretation can be given.

V. If local interpretation is feasible only under the conditions discussed, teleinterpretation must be cautioned against even more strongly lest it become mere 'guessing' interpretation.

The very term teleinterpretation, as far as I know, was coined by Graebner. He wrote an entire chapter (1911, pp. 62–70) introducing the concept. A few quotations: 'It is not justified to assume that identical expressions in separate locations must have the same meaning' (p. 63); 'It is not legitimate to interpret a number of facts which seem to be similar, mutually, if it cannot be made probable that they belong to the same culture' (p. 65); 'As long as it cannot be established that phenomena which are being compared have originated in the same culture' (p. 68), they may not be mutually interpreted. Teleinterpretation becomes permissible only if the phenomena which are being compared belong to related cultures. 'In the interpretation of any phenomenon only those ideas may be used which are present in the culture family to which the phenomenon in question belongs.'[26] Similarly, a decade later: 'The conclusion that a historical relationship exists will exert an influence upon the treatment of interpretation problems' (1919–1920, p. 1119). And when the Vienna Anthropological Society published in its *Proceedings* a paper which used Aranda customs to interpret Genesis genealogies (Posen, 1924). Graebner opposed this 'attempt' (1925, p. [15]) vehemently: 'I wish to stress emphatically that it is unmethodical to compare random tribes

25. For a linguistic parallel, see Sturtevant, 1947, p. 117f., on *shame-faced.*

26. Graebner, 1911, p. 167; see also p. 43 n. 1.!–The translation in Calverton, 1931, p. 425 of the last sentence quoted in the text above is wrong. The same wrong translation appears in W. Schmidt, 1939, p. 267. A correct translation (different from mine) in W. Schmidt, 1939, p. 268. – The German original of the sentences by Graebner can be found in Appendix 3.

living anywhere on the earth and to interpret them mutually without having first compared the complete cultural circumstances' (see Appendix 3 for the German text).

Graebner's dicta were misunderstood[27] from the very beginning.[28] Without any reason, *'"Ferninterpretation", "Kulturkreise"* and *"Kulturschichten"'* were lumoed together by Boas (1940, p. 303) who called them 'no less hypothetical than the 'Stufenbau' of Breysig or the sequences of Lamprecht'. What Graebner says about teleinterpretation is no hypothesis whatever but a number of don'ts; rules defining how to and, above all, how not to interpret. Boas himself, a few pages earlier (1940, p. 300) had referred to teleinterpretation as a 'methodological principle', yet claims it to be one of 'the arguments on which he [Graebner] bases his method of determining cultural relationships' (Boas 1940, p. 299f.). Definitely not. Neither local nor teleinterpretation is used by Graebner to determine cultural relationships. It is exactly the other way around: In the sentence quoted by Boas, Graebner insists that of 'two or more phenomena ... one may be used to interpret the other' (Boas' translation, 1940, p. 300) only if both belong to the same local culture or at least to a related culture (Graebner, 1911, p. 64). Boas closes by saying ([1940, p. 303) that he remains 'just as skeptical as before reading his [Graebner's] book in regard to the advisability of accepting Ratzel's "Ferninterpretation"'. Ratzel's? As far as I can see, the term *Ferninterpretation* does not appear in Ratzel.[29] The index to Ratzel's *Anthropogeographie* as well as that to his *Völkerkunde* do not list *Ferninterpretation* or even *Interpretation.*[30] In the entire chapter on teleinterpretation, Graebner does not

27. I have previously called this an 'unglaubliches Missverständnis' (Leser, 1954, p. 733). It hasn't helped a bit. After all, who reads book reviews?

28. Boas' review of Graebner's *Methode der Ethnologie,* 1911, originally was published in 1911 in *Science,* New Series, vol. 34, pp. 804–810. It is included in Boas 1940, pp. 295–304.

29. As stated in the text, I do not believe that the *term 'Ferninterpretation'* appears in Ratzel, but the *concept* most certainly does (the *correct* concept, of course, not the concept as misunderstood by Boas). See particularly Ratzel, 1904, p. 41, for what might be the strongest and clearest criticism of 'guessing' teleinterpretation: 'Der eine nimmt seine Analogien aus Afghanistan, der andere aus Indien, Amerika oder Neuseeland, wo er sie eben findet, keiner fragt, ob das nicht vielleicht dazu führe, den Keim mit der Blüte zu vergleichen.'

30. Ratzel, 1899, pp. 590 and 593; Ratzel, 1909 and 1921, pp. 390 and 392 (Ratzel, 1921, is an exact copy of Ratzel, 1909); Ratzel, 1912 and 1922, pp. 535 and 548 (Ratzel, 1922, is an exact copy of Ratzel, 1912. The first edition, Ratzel, 1891, contains no

once mention Ratzel (whom he so often otherwise cites). The term *Ferninterpretation* does not even appear in Bernheim to whom Graebner refers in his chapter on teleinterpretation.[31] *Ferninterpretation* is a new word coined by Graebner.

Actually, Boas himself, at least at times, warned against unwarranted teleinterpretation, just as had Graebner, by 'fighting ... the imposition of categories derived from our culture upon foreign cultures' (1940, p. 311).

Many years after Boas had published his review of Graebner, Herskovits similarly misunderstood Graebner. His translation of *Ferninterpretation* 'interpretation (of borrowing) despite distance' already contains the misunderstanding. Teleinterpretation is not an interpretation of borrowing but interpretation of culture traits, an attempt to understand the original meaning of a cultural phenomenon.

Herskovits writes (1951, p. 512, first published in 1948): 'Perhaps "interpretation (of borrowing) despite distance" best renders Graebner's idea. In accordance with this Graebnerian principle, if two elements of culture, material or nonmaterial, are logically identical when their outward similarity is established, the factor of distance can be disregarded. That is, he holds that if these elements are established on the basis of the criteria of form and quantity and to the satisfaction of the student, to be the same, they must derive from the same source, wherever they may be found.' Graebner taught the exact opposite, namely (I must repeat the quotation) that 'phenomena which are being compared' may not be mutually interpreted 'as long as it cannot be established that they ... have originated in the same culture' and that 'it is not legitimate to interpret a number of facts which seem to be similar, mutually, if it cannot be made probable that they belong to the same culture'.

---

index.); – Ratzel, 1885, pp. 651 and 652; Ratzel, 1890a, pp. 792 and 796; Ratzel, 1890b, pp. 760 and 763; Ratzel, 1894, pp. 728 and 731; Ratzel, 1895, pp. 767 and 769.

31. Bernheim, 1908, p. 820. On the other hand, the index to Bernheim, 1908, p. 824 itemizes *Interpretation* as follows: 'Interpretation, Definition und Aufgabe 485, 562, 566, engere Definition der Philologen 569, logisches Verfahren bei der I. 567, I. der Schrift 576, sprachliche I. 577, I. aus dem Charakter der Quellen (technische und generische) 584, aus Entstehungszeit und -ort ("historische") 587, aus dem Zusammenhang 601, 604ff., vgl. Komparative Methode; psychologische I. 589, 606, I. der Überreste 468, 570, 602, der Tradition 575, der Quellen durcheinander 599, subjektive und unmethodische I. 491, 498, 501, 578, 580, 586, 595ff.'

In 1950, Hodgen (p. 460), referring to Herskovits, but without reference to Graebner, claims that 'ferninterpretation ... as a principle of interpretation assumes an historical relationship between similarities widely separated in space in order to strengthen the case of the extreme diffusionist, and at times of the derivation of similarities from some single source. (*See* also Herskovits, 1949, pp. 409–514.)'.

Again, it is exactly the other way around: Only if an historical relationship between similarities (whether or not 'widely separated') does exist may they be interpreted mutually.

Hultkrantz, ten years later (1960, p. 97), quotes Hodgen without refuting her misunderstanding. Where he himself talks about teleinterpretation, he offers 'theory of diffusion at a distance' as a translation of Ferninterpretation (p. 125) and defines the concept of teleinterpretation as follows: 'The cultural interpretation, introduced by F. Graebner, according to which cultural complexes or elements widely dispersed but showing the same specific characteristics originate from the same historical source. This interpretation was used by Graebner and Schmidt and their disciples but is largely dropped in the modern Vienna school, at least in the form of identification between cultures in different continents (the F.-concept presupposes a more or less static conception of culture)' (p. 125). (See also Hultkrantz 1973, p. 178.)

Once again, I must state that what Graebner taught about teleinterpretation was not at all a 'theory of diffusion' (see Graebner, 1923, p. 447). He warned against the unmethodical interpretation of culture traits and condemned the use of traits from unrelated cultures for interpretation. To say that teleinterpretation presupposes 'a more or less static conception of culture' is as unwarranted as it would be to assert that etymology presupposes a more or less static conception of language. If Graebner had assumed a static conception of culture, he would have had no reason to warn against local interpretation.

Or am I perhaps the one who has misunderstood Graebner? There is a solid front against me: Boas, Herskovits, Hodgen, Hultkrantz – f. Actually, however, I have not interpreted Graebner; I have quoted him.

Graebner, who died in 1934 (and who had become hopelessly ill in 1926), could not have read what Herskovits, Hodgen and Hultkrantz wrote about teleinterpretation. He knew Boas personally and was well acquainted with many of Boas' publications; yet I am rather certain

that Graebner did not see Boas' review of *Methode der Ethnologie*. The review was not published in an anthropological journal, but in *Science*, which in 1911 was not one of the periodicals commonly read by European ethnologists.[32] Offprints of all papers discussing ethnological method in general and Graebner's, Ankermann's, Foy's contributions in particular which had come to the attention of Foy and Graebner were conspicuously displayed in the library of the Rautenstrauch-Joest-Museum[33] (where I read them) from 1920 to 1925 (and probably before and after those years as well). But Boas' review was not among them. During the time when Boas' review of Graebner's *Methode der Ethnologie* was published in *Science*, Boas frequently (it even seems, regularly) sent offprints of his articles to the Library of the Berlin Anthropological Society. (See, e.g., *Zeitschrift für Ethnologie*, vol. 42, 1910, p. 1001, #2 and 3; vol. 43, 1911, p. 187, #46–49; p. 386, #37; p. 389, #111; p. 630, #35; vol. 44, 1912, p. 906, #6; p. 907, #14 [this latter an offprint from *Science*, N. S., vol. 36, 1912]; vol. 45, 1913, p. 200, #6.) But since his review of Graebner's *Methode der Ethnologie* is not listed among the "Eingänge für die Bibliothek" I am assuming that Boas did not send out offprints of this review or copies of this particular issue of *Science*. Not once did Graebner, neither in class nor in private talks with me, mention the review. Nor did Foy. (Also, in a list of periodicals of interest to the ethnologist, compiled half a century later by the staff of the Rautenstrauch–Joest-Museum, *Science* is not mentioned.[34]

My main reason for claiming that my understanding of what Graebner taught about teleinterpretation is correct, and that Boas, Herskovits, Hodgen and Hultkrantz are wrong, is the fact that Graebner approved of my exegesis: Graebner, in 1924, read the manuscript of my book on the plow (which was not published until 1931). He knew and approved

32. That Boas, a quarter of a century earlier, in 1887 and 1888, for a year and a half, had been assistant editor of *Science* (see Herskovits, 1953, p. 12, and Rohner, 1969, p. 310) Graebner could not have known: In 1888 he was not more than eleven years old.

33. For the reader of 1975, it will be necessary to add that the Rautenstrauch-Joest-Museum in Cologne is the museum where Foy was the director and Graebner the curator. The library of the museum was and is one of the best anthropological libraries in the world.

34. Fröhlich, Bolz, and Stöhr, 1958, pp. 286–292. For a reference to the review as published in *Science*, see Kluckhohn, 1936, p. 165, n. 12.

the following passages in which I proclaimed, in the outspoken manner of the uninhibited youth, not sparing exclamation marks, how I understood his teaching on teleinterpretation:

'As the first and most essential principle of critique of sources Peisker recommends ... "to test the genuineness of the objects handed down to us, by means of the still existing and still used specimens". No! The objects handed down to us must first be subjected to penetrating critique of sources ...! The Peiskerian kind of critique of sources actually amounts o passing off an entirely inadmissible teleinterpretation as critique of sources.'[35]

'He [Peisker] uses the *possibility* of spatial and temporal teleinterpretation to dispel doubts raised by critique of sources! ... Teleinterpretation is not critique of sources. ... Teleinterpretation must be rejected unequivocally if it misleads to the vindication of source which, on the basis of critique of sources, is questionable.'[36]

'Only now, because it has become a probability that there is a relationship between the two areas involved may we use one source for the interpretation of the other. The opposite reasoning (i.e. to infer from the possibility of teleinterpretation that there is a relationship, or that the sources are reliable) must be ruled out.'[37]

VII. The 'lack of interest in psychology' which has characterized Swiss, Austrian and German anthropology (Heine-Geldern, 1964, p. 416) has its roots in the European opposition to 'naive' teleinterpretation. Any 'psychological' interpretation was suspect of being guessing teleinterpretation, i.e., of applying Western psychology, or Western ideas about

35. Leser, 1931, p. 35f.: '... Peisker ... empfiehlt ... als ersten und wesentlichsten quellenkritischen Gesichtspunkt ...: "die überlieferten Bildwerke mit Hilfe der noch vorhandenen aktiven Originale auf ihre Echtheit zu prüfen". Nein! Die überlieferten Bildwerke sind erst einer eingehenden Quellenkritik ... zu unterwerfen ...! ... diese Peiskersche Art der Quellenkritik ... kommt ... darauf hinaus ..., eine durchaus unstatthafte Art zeitlicher und räumlicher Ferninterpretation ... als Quellenkritik auszugeben.'

36. Leser, 1931, p. 105: '... er verwendet also die Tatsache der Möglichkeit einer räumlichen und zeitlichen Ferninterpretation zur Beseitigung quellenkritischer Zweifel! ... Ferninterpretation ist keine Quellenkritik ... Wenn sie [p. 106] zur Rechtfertigung einer aus im engeren Sinn quellenkritischen Gesichtspunkten zweifelhaften Quelle verleitet, ist sie aufs schärfste abzulehnen ...'

37. Leser, 1931, p. 464: 'Erst jetzt, wo uns der Zusammenhang wahrscheinlich geworden ist, können wir die eine Quelle durch die andere erklären ... Der umgekehrte Schluss, aus der Möglichkeit der Ferndeutung die Tatsache des Zusammenhanges oder der Glaubwürdigkeit der Quellen abzuleiten, ist unzulässig.'

primitive psychology, to a different culture. It remains to cite instances of inadmissible teleinterpretation and of methodologically correct teleinterpretation.

In an article about cancer, a recent German encyclopedia (*Brockhaus Enzyklopädie*, vol. 10, 1970, p. 160) writes: 'The oldest example of averting cancer by the lawgiver is ritual circumcision ... which, to a high percentage, prevents cancer of the cervix in the woman and which is virtually certain to prevent cancer of the penis in the man.' The words 'lawgiver' and 'ritual' refer to Moses, or the Mosaic law which was thus credited with having ordered circumcision millenia ago because in the mid-twentieth-century (*Encyclopaedia Judaica,* vol 5. p. 375) it came to be believed that persons circumcised soon after birth do not develop cancer of the penis and that women married to circumcised men develop cancer of the cervix only in extremely rare instances (Preston, 1970, p. 1855f.). (Alas, this twentieth-century theory today again has become open to question, and it is now claimed that neither the presence of the prepuce nor smegma is carcinogenic and that it is not circumcision that prevents cancer of the penis or cervix; Preston, 1970, pp. 1853–1858; de Jong, 1971; Rikin, 1973, pp. 115–117.)

It could be argued whether this reason ascribed to the Mosaic law for the introduction of circumcision actually is an example of unsound teleinterpretation or rather should be considered a folk interpretation (an 'invented explanation') (see above p. 000). However, since this explanation claims a theory devised in a foreign culture (a theory which did not exist at the time of the introduction of circumcision) to be the reason for the origin of the custom, we here are faced with a particularly drastic example of impermissible teleinterpretation.

In 1967 Philip Singer and Daniel E. DeSole published a paper (Prepared for the American Anthropological Association 65th Annual Meeting, Pittsburgh, November 17–20, 1966[38]) in which they stated

38. Singer and DeSole, 1967, p. 358 n. 1. – The paper by Singer and DeSole seems to have rekindled the general interest in subincision, see Abernethy, 1972, and Anthropology Student Seminar, 1973. When Segal 1972, p. 24 (to whom Abernethy refers) claims that through subincision 'the semen flow, mainly, is diverted and lost' (reading that I cannot avoid being reminded of what Philo said about the effects of circumcision and of the lack of circumcision on population growth, see above p. 000), it must be stated that that old fable has been disproven long ago (see, e.g., Spencer and Gillen, 1899, p. 264; Stoll, 1908, p. 529; Montagu, 1937–1938, pp. 194–198; Berndt and Berndt, 1964, pp. 124, 145) and that, contrary to Abernethy, 1972, p. 12, subinci-

(p. 355) that subincision 'still does not have an acceptable explanation' and that 'what Ashley Montagu said ... in 1937 is still true in 1966: ... "no theory which has thus far been advanced to account for the meaning of subincision has attained even to the status of a remote degree of probability"'. Having thus discarded (rather cavalierly) the explanation which Ashley Montagu offered in 1937–1938, the authors then say (p. 356) that they 'would like to offer another explanation'. Since they do not refer anywhere to the article by Cawte, Djagamara and Barrett published in 1966 (pp. 245–253; see also Cawte, 1968, pp. 961–964!), it will be permissible to treat the paper by Singer and DeSole exclusively on its own merits without taking into consideration the article by Cawte, Djagamara and Barrett (which contains information suitable to alter considerably the appraisal of Singer and DeSole's explanation). My concern is methodology not the kangaroo hypothesis nor subincision in general.

Singer and DeSole explain 'the subincision ritual ... as an increase ritual[4] with sexual aspects based on the keen observation of the aboriginal that the kangaroo has a bifid penis, with the outer surface comparable to an externalized urethra'. That such an observation actually was made

---

sion does not 'limit the reproductive potential of a society'. Since today it seems to be of paramount importance to 'limit the reproductive potential' everywhere in the world I hasten to add that I only can hope that those who tend to believe unproven or doubtful or demonstrably wrong (take your pick) interpretations will prefer Philo to Segal and will simply prohibit circumcision everywhere instead of forcing subincision upon all mankind. (The effects would be exactly the same, namely none.) Nonetheless, Sam grateful to Abernothy for having referred me to Segal who in turn referred me to the outstanding study by Himes which I had missed entirely. Has Segal not misquoted Himes in a rather strange manner, I might not have made an attempt to look up the quotation (for which I intended to blame Himes). Segal writes: 'Even primitive societies have developed ways of preventing sperm transport: Aberiginal Australians, it is reported, create a urethral fistula at the base of the penis with a sharpened twig during puberty rites. The semen flow, mainly, is diverted and lost.[21] (During micturation the hole is covered.) (Reference 21 refers the reader to Himes 1936.) No such passage can be found in Himes who, to the contrary, gives detailed (and correct) quotations both from the sources and the literature. (I wonder where Segal found the sentences which he ascribes to Himes, and what the source is for the parenthesis which Segal has added but for which no reference is given.) Himes' work is a comprehensive anthropological and historical study which should have caught the attention of anthropologists, but it appears that almost the entire anthropological profession overlooked it although the book contains important information which Himes personaly elicited from Radcliffe-Brown, Linton, and W. Lloyd Warner. (Himes, August 4, 1899–June 6, 1944, taught at Cornell, Simmons, Clark, Colgate, and at the Amerıcan University at Biarritz France, according to *Who Was Who in America*, vol. II, 1943–1950, p. 254.)

by the 'aboriginal' is not documented by the authors (their footnote 4 following the word 'ritual' does not offer any such documentation). That 'the keen observation of the aboriginal' is nothing but the authors' personal interpretation is admitted in the very next sentence in which it is stated that 'it is most unlikely' that the observation would not have been made. Thus the explanation offered by Singer and DeSole rests on an assumption of two western scholars and therefore must be characterized as a mere guess, an inadmissible teleinterpretation.

However, we are faced not only with teleinterpretation in the sense that the explanation of an Australian custom is based on an assumption of two American scholars but also with teleinterpretation in the sense that various Australian tribes are used for mutual interpretation without even the slightest investigation having been made as to whether these tribes are culturally related:

(a) In footnote 4 Singer and DeSole refer to the Aranda, in the text on p. 358 to Berndt and Berndt's *Sexual Behaviour in Western Arnhem Land*. The distance separating the Aranda territory from Western Arnhem Land is 700 to 800 miles.

(b) Singer and DeSole do not tell us which tribe or tribes they wish to credit with having instituted subincision and with the 'keen observation of the aboriginal'. This very wording forces us to ask the question: Who is 'the aboriginal'? Any member of any one of the approximately five hundred (Tindale 1940; Berndt and Berndt, 1964, p. 28) tribes of Australia[39] (a continent of three million square miles, thus roughly the size of the 48 contiguous States!)? Their cultures, as well as their languages, differ dramatically (Berndt and Berndt, 1964, p. 24). Many of them never practiced subincision.

(c) Singer and DeSole (p.357f.) write: 'It has been claimed that the purpose of the subincision is to imitate the vulva. ... In the best ethnographic description of Australian aborigine sexual behavior (Berndt and Berndt, 1951), there is ... no mention ... of the penis as a vulva or, in fact, of the penis resembling a vulva in either form or function. If, indeed the penis is called vulva, as is claimed by some investigators, then it would seem that there would be some mention of it in sexual behavior.' This last sentence, in my opinion, could be read in two different ways: (1) It could be assumed that an oversight in proofreading

39. The names of 128 tribes are given by Vatter (1925, pp. 24–26); of 389 by Róheim (1926, pp. 29–32): of 180 by Berndt and Berndt (1964, pp. 28–34); and of more than 500 by Tindale (1940).

prevented the last two words from being printed in italics and from being capitalized; then the end of the sentence would read: '... mention of it in *Sexual Behavior*', thus referring to the book by Berndt and Berndt. (2) Or, assuming that the intentions of the authors were not distorted by misprints, the meaning then would be that the authors wished to stress that 'if the penis is called vulva ... some mention of it' would have been made in some source reporting on the sexual behavior of Australians.

I cannot believe that (2) could be the correct assumption because there are several sources available which state that in actual sexual behavior the subincised penis is used as a (kind of) vulva (e.g., Roth 1897, pp. 35, 180; Klaatsch, 1907, p. 581f.; Karsch-Haack, 1911, pp. 77–80; [see also Blüher, 1953, p. 333!]; Campbell, 1968, p. 154, n. 98). If, however, (1) should be the case, then Singer and DeSole in the sentences quoted would have intended to discredit the claim 'of some investigators' that 'the penis is called vulva' by maintaining that such a claim would have had to be mentioned in the book by Berndt and Berndt. Such reasoning would be based on methodologically incorrect teleinterpretation: There is no necessity for authors who write on *Sexual behavior in Western Arnhem Land* to 'mention ... the penis as vulva ... or ... resembling a vulva in either form or function' if such is the case only in other parts of Australia but not in Western Arnhem Land. If a book dealing with the Plains Indians does not refer to the art of the Northwest coast (or if in my description of the agriculture of the Nyakyusa [Leser, 1960b] I do not mention Zaire agriculture) no conclusion may be drawn from such non-reference, and anyone who would draw such conclusion would be guilty of unwarranted teleinterpretation. That Singer and DeSole emphasize that Berndt and Berndt do not mention what otherwise could have been expected could be because the Berndts are considered by Singer and DeSole to have written 'the *best* ethnographic description of Australian aborigine sexual behavior' and that therefore such an omission in any other writer's work would be less significant. If this should be the case, I would have to state that in Ronald M. Berndt's *Kunapipi* (1951) the following sentences are found: '... In those areas [Rose River and regions to the South] the incisure was originally made of the Wauwalak (or ... the Kunapipi ...) to represent their vaginae' (p. 110); '... the incisure is said to represent the Kadjari's vulva' (p. 187).

However, Singer and DeSole must be criticized not only for engaging in fanciful teleinterpretation but also for engaging in local interpreta-

tion: They consider only that form of subincision in which the penis is split from the scrotum to the meatus and base their assumption on the similarity of a penis subincised in that way with the bifid penis of the kangaroo (p. 356, fig. 1). If the extreme form of subincision is claimed to result in a similarity to the kangaroo penis, then there is no explanation for the other forms present in Australia. The 'form of subincision which is most commonly practiced in some parts of western Australia' consists of 'a small incision ... made in the urethra immediately anterior to the root of the scrotum' (Montagu, 1937, p. 294).

Finally, the kangaroo hypothesis cannot possibly explain subincision outside of Australia where there are no kangaroos, and Singer and DeSole therefore should have examined the problem of whether subincision outside of Australia might belong to a genetically related culture (as it has been claimed, e.g., for Fiji.[40] Outside of Australia there would have been no chance for anyone, even if endowed with 'the keen observation of the aboriginal' (Singer and DeSole, 1967, p. 356) to see the penis of a kangaroo, to say nothing of witnessing the kangaroo urinate and copulate (Singer and DeSole, 1967, p. 356).

Some readers might consider what I have said so far to be self-contradictory; on the one hand I have accused Singer and DeSole of incorrect teleinterpretation for lumping together Western Arnhem Land and the Aranda. Yet, at the same time I have blamed them for too narrow a local interpretation in not having taken into consideration certain parts of Western Australia and Fiji; and this I have done, even though the distance from the Aranda area to Fiji is twice the distance from the Aranda to Western Arnhem Land! So, to some, my objections may seem unfair, if not outright ridiculous. The opportunity to repeat my main points in order to explain this pseudo-contradiction was my principal reason for choosing Singer and DeSole's paper for discussion.

A study merely describing the culture (or some of the culture) of any area is under no obligation to look beyond the area being described. But a study attempting to *explain* a culture, or a culture trait, may base its explanation on one single, restricted area (or even a number of areas) without considering the entire distribution of the culture or culture trait in question *only* if it has been determined that it (whether an

40. Graebner, 1905, pp. 32, 40, 42; 1908, p. 666f.: 1909a, p. 161; 1909b, pp. 733, 734, 736; 1911, p. 168; 1923, pp. 450, 452; Rivers, 1908; 1926, p. 68f.; Schmidt, 1908; Montagu, 1937–38, p. 201.

entire culture or a culture trait) is a purely local feature. Therefore, the failure of Singer and DeSole to ascertain that the form of subincision on which they base their theory exclusively is not related to other forms of subincision constitutes a methodological error: unjustifiable local interpretation.

On the other hand, a study attempting to explain a culture or a culture trait may not make use of cultures or culture traits which are unrelated; 'in the interpretation of any phenomenon only those ideas may be used which are present in the culture family to which the phenomenon in question belongs' (above p. 000). Therefore, references combining the Aranda and Western Arnhem Land (without making any attempt to prove that their cultures are related but by apparently taking it for granted that any group in Australia is comparable to any other group in Australia) constitute a methodological error: guessing teleinterpretation.

Thus, on the one hand one may not base an interpretation of a culture on facts taken from another, or several other, cultures if it cannot be made probable that the cultures in question are related. On the other hand, in interpreting a culture, one may not ignore facts belonging to related cultures.

I have used the paper by Singer and DeSole merely as a handy example, mainly of methodologically incorrect teleinterpretation. It is not within the scope of what I had intended to say to discuss the origin and the meaning of subincision. Therefore, comments on the article by Cawte, Djagamara and Barrett (which was published a short time earlier) would not be required. Yet the material presented in this earlier article (as well as in other works, e.g., Klaatsch, 1907, p. 582, n. 12, last par.; Karsch-Haack, 1911, p. 69; Strehlow, 1913, p. 35 [illustration with caption]; Basedow, 1927, p. 146; Winthuis, 1928, p. 39; Porteus, 1931, p. 281), had it been used, would have given considerable support to the Singer and DeSole hypothesis.

Cawte, Djagamara and Barrett offered their hypothesis on the origin of subincision in such a cautious manner (see particularly p. 251) rejecting so expressly 'simplistic' theories (Cawte, 1968, p. 963) that no methodological objection to their presentation is warranted. Saying that 'no single explanation is fully satisfactory' (Cawte, Djagamara and Barrett, 1966, p. 246) and that they themselves do not claim their hypothesis 'as the linear cause of subincision' (p. 252, column a), they called subincision in Australia 'complexly overdetermined' (p. 252, column a) and

'complexly determined' (p. 252, column b) and stated that 'the evidence relating to the spread of subincision is inconclusive' (p. 252, column a). Cawte, Djagamara and Barrett carefully combined folk interpretation (kangaroo myths) with local interpretation (giving an explanation for the existence of the custom among the Walbiri) and also took into account teleinterpretation by referring to the different Australian 'regions' where 'ritual subincision occurs' (p. 252, column a) and even to the distribution of subincision outside Australia (p. 245). Among the objections which might be raised against their paper, in my opinion, would be that the sources (Roth, 1897, pp. 35, 180; Klaatsch, 1907, pp. 581–582; Strehlow, 1913, p. 98) and the literature (e.g., Karsch-Haack, 1911, pp. 77–78; Winthuis, 1928, pp. 39–42; Montagu, 1937–1938, p. 205; Baumann, 1955, pp. 212–217; also Graebner, 1911, p. 168, n. 5; Basedow, 1927, p. 148, etc.) stressing the double-sex aspect of subincision and discussing the homosexual implications have been somewhat neglected and that the authors do not seem to be aware of the old claim of a close cultural relationship between Fiji and certain parts of Australia.[41]

VII. Chapter 5, verse 2 in the Book of Joshua reads: 'At that time the Lord said to Joshua: "Make flint knives and circumcise the people of Israel again the second time"'. Since it was believed to be scarcely possible to circumcise a circumcised man 'again the second time' there have been many efforts made to explain this verse.

The earliest attempt is to be found in the Book of Joshua itself in

41. See above note 34a. — Cawte, Djagamara and Barrett (1966:251a) do mention hypospadias as 'a not uncommon congenital malformation'. Chen and Woolley (1971:153) call hypospadias 'one of the most common urogenital anomalies'. Has any research been done in trying to find out how often hypospadias occurs among the Walbiri (and among other Australians)? (See also Stuart's remark, 1896:121.) It seems that we do not even know how often hypospadias occurs in other populations. The estimates vary so greatly (and in many instances the samples on which the estimates are based are so small) that they seem to prove only how little we know: Lenz, 1921, p. 182, estimated 1 in 300; Netter, 1954, p. 29, estimated 1 in 500; Stern, 1960, p. 409, estimated 1 'in every few hundred births'; Campbell and Harrison, 1970, p. 1599, reported estimates ranging from 1 in 1,800 to 1 in 160; Aarskof, 1970, p. 32, reported estimates from 0.55 per thousand to 6.25 per thousand; Horton and Devine, 1972, p. 6, claimed 'about 1 in every 300 to 400 boys born in the United States'; The Institute for Sex Research (Beasley, 1973) found 80 cases of hypospadias in 8274 white males and 17 cases in 1088 black males. The latter would amount to 1 in every 64 male births. The 'incidence of 305 cases of hypospadias per 10.000 births for Caucasian boys' (Chen and Woolley 1971:153) is due to a misprint, the figure should read 30.5, see Chung and Myrianthopoulos 1968: 48.

the verses 4 to 7 in which it is said that the people who had been born in Egypt and had been circumcised had died and that 'it was their children', the people 'born on the way in the wilderness', who 'had not been circumcised' whom 'Joshua circumcised'.

Verses 4 to 7, however, are considered by many biblical scholars as an interpolation (Sherman, 1909, p. 119; *Interpreter's Bible*, 1953, vol. 2. p. 574; *Peake's commentary*, 1962, p. 293). For several reasons, among them the fact that the Septuagint 'translates verse 2 so as to omit both *again* and *the second time*' (*Interpreter's Bible*, 1953, vol. 2, p. 573), it has been argued that 'the rite of circumcision was here introduced into Israel for the first time' (*Interpreter's Bible*, 1953, vol. 2. p. 573). There is no need, in this context, to examine the attempts to explain Joshua 5:2 or to enter into a discussion of the difficulties of the text and the details of Old Testament criticism.[42] Yet, Joshua 5:2 offers an opportunity to cite an example of methodologically correct teleinterpretation.

During the past seventy years so many coincidences between Hebrew culture as depicted in the Old Testament and some African cultures have been brought to view that today an old stratum common to Hebrews and certain African peoples must be considered to have been well established. Back in 1904, Moritz Merker collected such a large number of parallels between Masai and Old-Testament culture that he felt justified in claiming that Hebrews and Masai (and some other peoples) were closely related. When first published, Merker's theories and even some of his material were criticized (Meinhof, 1904; Ankerman, 1905a), but his work later was vindicated (Hommel, 1910; ten Raa, 1970). Other scholars added more African-Hebrew parallels.[43] If there is a close historical relation between Masai and Hebrews, it then becomes permissible mutually to interpret Masai and Hebrew cultures.

In Masai circumcision the foreskin is not cut off entirely (Widenmann, 1895, pp. 302–303; Johnston, 1886, p. 412f.; 1902, vol. 2, p. 804; Bagge, 1904, p. 168f.; Merker, 1910, p. 62). After 'the place on the prepuce

42. Just a few references: *A Catholic commentary on the holy scripture*, 1952, p. 282, # 230b; *Jerome biblical commentary*, 1968, p. 128f.; *Realencyklopädie für protestantische Theologie*, 1897, p. 661; Sherman *(Schaff–Herzog Encyclopedia)*, 1909, p. 119; Martin Noth, 1938, p. 17; F. Stummer, 1954, p. 162; Hans-Joachim Kraus, 1966, pp. 152–159; H. Wheeler Robinson, no date, p. 287f.

43. Lindblom, 1926; Bryk, 1928; Patai, 1947; Jensen, 1953, 1960a; Patai, 1962; Isaac, 1964a, 1964b. Some of the parallels compiled by Guptill, 1927, may also belong into this category.

where the circumcision is to be performed' has been marked by 'a white line', the 'prepuce is retracted and the operator scarifies each side of the *frenum* with the point of his knife, by means of which a certain amount of play is allowed. Inserting his finger between the upper surface of the glans and the prepuce he makes a transverse incision immediately below the part marked with the white paint, which is at a level of the corona glandis. Through this opening he protrudes the *glans penis*, and by means of a thorn of the Ol-Oimoronyai tree so pierces the skin of the prepuce that it is unable to return to its former position; then, if this part of the prepuce appears to be too long, he cuts off a small portion from it, ...'.[44]

Had Hebrew circumcision originally resembled Masai circumcision, it would have been possible to circumcise a previously circumcised man 'a second time', this second time cutting off the foreskin entirely. This

44. Bagge, 1904, pp. 168–169.–I should like to add Widenmann's description because he was a medical doctor who had the opportunity of learning how to perform the operation in the orthodox Masai way and who has himself performed the operation under Masai supervision (The uncircumcised youngster is called 'leion'): 'I. Der Leion sitzt mit gespreizten Beinen auf der Erde und wird von einem hinter ihm sitzenden älteren Kameraden gestützt, der zugleich Assistenz an der Wunde leistet. Der Operateur, der ihm gegenüber sitzt, streicht erst sich selbst, den Leion, den Assistenten und etwaige nähere Umgebung mit Kalkerde an, die er in einer Lederschachtel bei sich führt, und zwar macht er je 1 Strich auf die Stirn, an die Schläfen und auf den Bauch. Er zieht nun das Praeputium ein paar Mal gehörig in die Länge und trennt dann – irgend eine Reinigung geht nicht voraus – mit einem zweischneidigen Messer von etwa 6–8 cm Schneidelänge das Frenulum durch und schneidet weiterhin zu beiden Seiten, circulär an der Umschlagsfalte, das innere Blatt des Praeputium los, so dass sich dieses allmählich nach vorn zurückstreifen lässt, wobei er durch seichte Incisionen in das dünne Zellgewebe zwischen den beiden Blättern die Ablösung befördert, bis die innere Fläche des äusseren Blattes auf diese Weise als Wundfläche zu Tage liegt. Das Praeputium wird dadurch immer länger. Zum Schlusse wird der in das Praeputium wie in einen Handschuhfinger eingelegte Zeigefinger mehrmals reibend zurückgezogen, um die Innenseite zu glätten und die Blutgerinnsel abzustreifen.

II. Nun wird der Zeigefinger der linken Hand wieder in die Hülse des straff angezogenen Praeputium eingelegt und auf der Dorsalseite auf die vorgedrängte Fingerkruppe ein kleiner Querschnitt in das Praeputium gemacht, so dass ein Schlitz entsteht, der dann durch kleine Incisionen so weit dilatirt wird, bis sich die Glans penis mit Mühe durch denselben nach oben durchstecken lässt. In dieser neuen Lage verbleibt nun die Glans, während das ganze Praeputium als langer Wulst nach unten und vorn herabhängt.

III. Zum Schlusse wird das vorderste Ende des Praeputium, da dasselbe durch die vorhergegangene Ablösung des inneren Blattes in der That sehr verlängert ist, in einer Ausdehnung von etwa 0,5 bis 1 cm mit einen queren Schnitte abgetrennt.

Ein Verband wird nicht angelegt. Die Beschnittenen reinigen den Penis mit Milch und trocknen ihn mit den weichen Blättern einer Labiate ab.'

assumption would explain the obscure passage in Joshua 5, as Merker (1910, pp. 329–331) and Isaac (1964a, p. 453) suggested, and even explain Joshua 5, verse 9 (see Merker, 1910, p. 330; Isaac, 1964a, p. 453). It is only the likelihood that Masai and Hebrew cultures are genetically related which permits such teleinterpretation.

This methodologically correct teleinterpretation, then, can be strengthened by calling forth those Jewish traditions which may indicate that circumcision had not always removed the foreskin entirely.[45]

The probability that certain African cultures are related to the culture represented in the Old Testament makes it feasible to interpret other Old Testament passages by using material from those African cultures and vice versa (Jensen, 1953; Isaac, 1964a, 1964b). Although the words 'You shall not boil a kid in its mother's milk' appear three times in the Old Testament (Exodus 23:13, 34:26; Deuteronomy 14:21) they seem to be a rather insufficient basis for the rigid rule that no kind of meat may be cooked together with milk, that 'one may not eat milk after meat in some meal' and that 'strict observance demands an interval of as long as six hours between eating meat and dairy dishes' (*Encyclopaedia Judaica*, 1971, vol. 5, p. 42). Commentaries try to explain the prohibition as a 'rejection of [a] pagan custom': 'In the polytheism of Canaan and Mesopotamia it was an accepted practice to prepare a sacrifice by cooking it in milk. The law here is evidently a rejection of the pagan custom in order to avoid obvious imitation. It is the basis of the separation of meat and milk foods in later Judaism' (*Interpreter's Bible*, 1953, vol. 2. p. 424).

But the fuct is that the Masai may not consume milk and meat at the same time either (Merker, 1910, p. 33). If Masai and Hebrew cultures are related, then this convention should be explained not as 'a rejection of the pagan custom' but as a custom which is common to Hebrew and Masai cultures, and as only one specific instance of a far more general law.

VIII. Erroneous interpretations frequently tend to mistake the effect for the cause by claiming an accidental side-effect to have been the reason for the introduction of a custom, although no such effect could possibly have been foreseen at the time of the origination of the custom.

45. *Real encyklopadie für protestantische Theologie*, 1897, p. 660; Gray, 1911, p. 660; Bryk, 1931, pp. 31–38. On the basis of a passage in Hoffmann, 1822 (p. 266, column b, lines 3–8, with a reference to Buxtorf, 1639) I am inclined to assume that the foregoing interpretation of Joshua 5, 2 perhaps already is a very early one.

A folk interpretation teaches us what informants believe about their culture and therefore is of interest to the anthropologist. Yet it is no more correct an interpretation than is a folk etymology a true etymology.

Local interpretation, arrived at by the anthropologist on the basis of a careful examination of a narrowly local area, without taking into consideration a possibly wider distribution of the culture which is being studied, corresponds to an etymology based exclusively on one dialect, ignoring insights which would be gained from a study of related dialects and languages.

Teleinterpretation must be rejected if it uses examples taken from unrelated cultures to explain the meaning of phenomena in the culture which is being studied; it is comparable to an etymology which would explain the meaning of a word in one language by reference to a seemingly similar, but in reality unrelated, word in an unrelated language. Methodologically correct teleinterpretation interprets phenomena in one culture by studying the meaning of similar phenomena in related cultures and thus is comparable to 'scientific etymology' (Sturtevant, 1947, p. 118).

IX. Having written about Scandinavian and continental research, I am tempted to add a few words about two English savants. Over and over again I have referred to the importance of working toward a genetic classification of cultures. The method leading to such classification is due primarily to these two Englishmen. It is the same method which, over the past two hundred years, has enabled linguists to arrive at a genetic classification of languages and which, for more than a hundred years, has been used by biologists to work out a genetic classification of organisms. Sir William Jones, in 1786, stated that Sanskrit, Greek and Latin show 'a stronger affinity ... than could possibly have been produced by accident; so strong indeed, that no philologer could examine them all three without believing them to have sprung from some common source, which, perhaps, no longer exists'.[46] Waterman, from whom I have taken this Jones quote[47] adds (1936, p. 16): 'Scholars before

46. Waterman, 1963, p. 16. See also the recent corresponding statement by Bornemann, 1973, p. XIV: 'Diese Sprachen sind unter sich urverwandt, d.h. sie zeigen ... eine weitgehende Übereinstimmung, die nur durch die Annahme erklärt werden kann, dass sie sich alle aus einer gemeinsamen, freilich nicht mehr direkt erhaltenen Grundsprache ... heraus entwickelt haben.'

47. The same quote, of course, can be found in many other publications, see, e.g.. Edgerton, 1966, p. 5f.; Trager, 1972, p. 148; Fromkin and Rodman, 1974, p. 236,

Jones had of course noticed the similarities ... but ... no one prior to him had reached that conclusion – arrived at not by intuition but by inspection of data – that these resemblances must be due to a common descent from a hypothetical earlier language "which, perhaps, no longer exists".'

Less than a hundred years later, Charles Darwin in *The descent of man* stated that those 'anthropologists ... who do not admit the principle of evolution, must look at species as separate creations' (1878, p. 176, part I, ch. VII, paragraph following n. 20) and further said about 'the existing races of man' that 'they are found to resemble each other closely in a multitude of points. Many of these are of so unimportant or of so singular a nature that it is extremely improbable that they should have been independently acquired' (1878, p. 178, part I, ch. VII, paragraph preceding n. 24).

Whenever two or more cultures resemble each other closely in a MULTITUDE of points, MANY of which are of so UNIMPORTANT a nature that it is extremely improbable that they should have been independently acquired, even anthropologists finally will have to go along with Charles Darwin and Sir William Jones and admit that these resemblances must be due to a common descent from a hypothetical earlier culture 'which, perhaps, no longer exists'.

APPENDIX 2

The terms folk, local, and teleinterpretation require an explanation.

The term *folk interpretation* has been coined as an analogue to 'folk etymology' ('...an incorrect popular notion of the origin of meaning ...', *American Heritage Dictionary* p. 509).

Local interpretation refers to an interpretation that is based exclusively on a single, restricted area in which a cultural phenomenon is found (although this phenomenon may have a considerably wider distribution).

In this age of telephones, telegrams, TV, telephoto, telepathy, telelenses, telecommunications and more than twenty other words listed in dictionaries (e.g., *American Heritage Dictionary*, 1322–1324; *Random House Dictionary*, 1459–1460; *Webster's Third New International Dictionary*, 2349–2351) as having the Greek *tele* as one of their component parts, *teeinterpretation* (Leser, 1962a, p. 856; 1962b, p. 357) seems to me to be/more easily understood, less unwieldy and less misleading than the

terms 'interpretation despite distance' (Herskovits, 1951, p. 512), 'far-reaching interpretation' (W. Schmidt, 1939, p. 96), 'distant interpretation' (W. Schmidt, 1939, pp. 125, 131ff.) and 'distant inferential interpretation' (W. Schmidt, 1939, p. 135), which previously have been used as translations for the German *Ferninterpretation.* Teleinterpretation in anthropology corresponds to etymology in linguistics. As etymology studies the *origin* and semantic *derivation* of words so does teleinterpretation study the origin and meaning of cultural phenomena. And as linguists distinguish between 'fanciful' or 'guessing' etymology (Müller, 1877, pp. 267, 263–266) (which must be refuted [Müller, 1877, p. 267]) and 'sound' or 'scientific' etymology (Müller, 1877, p. 267; Sturtevant, 1947, pp. 117–119), so in anthropology 'fanciful' or 'guessing' teleinterpretation must be distinguished from methodologically correct teleinterpretation: 'Guessing' teleinterpretation interprets phenomena of a given culture through data from unrelated cultures (see note 24). Methodologically correct teleinterpretation uses data from genetically related cultures (even if geographically or chronologically remote) and thus is the equivalent of 'sound' or scientific etymology, an etymology which 'is obtained by finding ... the forms in related languages which are divergent variants of the same parent from' (Bloomfield, 1933, p. 15).

APPENDIX 1

Slotkin (Hempel):	*History and Theory:*	*Schiller:*	*Suggested translation:*
upon distant oceans and along distant coasts	in distanc oceans and on remote shores	in fernen Meeren und auf entlegenen Küsten	in distant seas and on far-off shores
occupying the most varied degrees of culture	at various levels of development	auf den mannigfaltigsten Stufen der Bildung	on the most varied stages of evolution
these rude tribes	these savage tribes	diese rohen Völkerstämme	these crude tribes

Slotkin (Hempel):	*History and Theory:*	*Schiller:*	*Suggested translation:*
sufficiently advanced in civilization	progressed sufficiently in our own civilization	in unserer eigenen Kultur weit genug würden fortgeschritten sein	would have progressed sufficiently in our own culture
to restore	to recover	wieder herzustellen	to reconstruct
these tribes	these peoples	diese Völker	these peoples
not the first degrees	not even at the earliest level	nicht einmal die erste Stufe mehr	not even at the earliest stage
a much lower creature	even more pitiful	noch verächtlicher	even more contemptible
These tribes already constitute political bodies, peoples; it is only by extraordinary exertions that man was enabled to form a political society. ...	What we find are already peoples – political societies – but man must have achieved social organization only as a result of extraordinary exertions.	Wir finden jene doch schon als Völker, als politische Körper: aber der Mensch musste sich erste durch eine ausserordentliche Anstrengung zur politischen Gesellschaft erheben.	The former, after all, we meet already as peoples, as body politic: but first, man, by exerting extraordinary effort, had to progress to political organization.

Appendix 3

Graebner on local interpretation:

1911: 168: 'Im einzelnen ist eine Erklärung aus lokalen Kulturverhältnissen ... nur bei solchen Elementen und Formen zulässig, die sich kulturgeschichtlich als lokale

Sonderbildungen charakterisieren. Doch muss der Lokalcharakter auch wirklich gesichert sein.'

1909: 363: 'Ebenso sind Interpretationen von Erscheinungen, z. B. Mythendeutungen, von einzelnen Lokalformen her immer von zweifelhafter Gültigkeit.'
See also above, note 18.

Graebner on teleinterpretation:

1911: 63: 'Es ist ein ... nicht gerechtfertigter Schluss, dass gleichen Äusserungen auch der gleiche Sinn zugrunde liegen müsse ...'

1911: 65: 'Ich bin nicht berechtigt, mehrere gleichartig scheinende Tatsachenkomplexe zu gemeinsamer Deutung zusammenzufassen, wenn ich nicht wahrscheinlich machen kann, dass sie der gleichen Kultureinheit angehören.'

1911: 68: 'Solange dieser Nachweis nicht geführt ist', 'dass die verglichenen Erscheinungen in einem kulturellen Zusammenhange zueinander stehen ...'

1911: 167: 'Jede Erscheinung ist nur aus den Ideen der Kulturgruppe abzuleiten, der sie als Element angehört.'

1919–1920: 1119: 'Die Annahme der kulturgeschichtlichen Beziehungen wird dann natürlich auf die Fragen der Interpretation weiterwirken.'

1925: 15: '... lege ich ... Gewicht darauf, zu betonen, dass eine Vergleichung beliebiger Völker auf der Erde und ihre gegenseitige Interpretation, ohne dass eine Vergleichung der ganzen Kulturverhältnisse vorangegangen ist, unmethodisch ist.'

References

Aarskof, Dagfinn (1970), Discussion, 1. Etiology of hypospadias, *Acta Paediatrica Scandinavica.* Supplement 203: 32–35.

Abernethy, Virginia (1972), Subincision again. *Newsletter of the American Anthropological Association,* 13 (10): 12.

Adler, Bruno (1901), *Der nordasiatische Pfeil.* Inaugural-Dissertation, Universität Leipzig, Leiden, E. J. Brill.

— (1902), Die Bogen Nordasiens, *Internationales Archiv für Ethnographie,* 15: 1–27.

American Heritage dictionary of the English language (1969), William Morris (Ed.). Boston, Houghton Mifflin Company.

Anders, Ferdinand (1965), Professor P. Wilhelm Koppers SVD: Bibliographie. *Wiener Völkerkundliche Mitteilungen,* 12th year, new series vol. 7. Pp. 1–17.

Andree, Richard (1881), Die Beschneidung, *Archiv für Anthropologie,* 13: 53–78.

Anell, B., and Lagercrantz, Sture (1958), *Geophagical customs.* Uppsala, Almqvist & Wiksells Boktryckeri AB.

Anell, Bengt (1969), *Running down and driving of game in North America.* Lund, Ethnographica Upsaliensia 30.

Ankermann, Bernhard (1901), Die afrikanischen Musikinstrumente, *Ethnologisches Notizblatt,* 3: 1–134.

— (1905a), Review of M. Merker, Die Masai, Berlin, 1904, *Zentralblatt für Anthropologie,* 10: 286–288.

— (1905b), Kulturkreise und Kulturschichten in Afrika, *Zeitschrift für Ethnologie,* 37: 54–84.

Anthropology student seminar (1973), Australian fantasy: A joint effort of the anthropology student seminar, University of Toledo, *American Anthropologist,* 75: 904.

Ashley-Montagu *see* Montagu, Ashley.

Atlas över Svensk Folkkultur (1957), *see* Erixon, 1957.

Bagge, S. (1904), The circumcision ceremony among the Naivasha Masai, *Journal of the Anthropological Institute of Great Britain and Ireland,* 34: 167–169.

Balassa, Iván (1972a), *Getreidebau in Ost- und Mitteleuropa.* Budapest, Akadémiai Kiadó.

— (1972b), Die Haupttypen der Holzpflüge in Ungarn im 18. und 19. Jahrhundert. In Balassa, 1972a. Pp. 441–485.

Balogh, István (1972), Geräte und Verfahren der Ernte in den zentralen Gebieten des Karpatenbeckens im 19. Jahrhundert. In Balassa 1972a. Pp. 529–559.

Barabás Jenő (1963), *Kartográfiai módszer a néprajzban.* Budapest.

— (1972), Diffusionen im Bauwesen des Karpatenbeckens. In: *Festschrift Matthias Zender*. Pp. 227–236.

Barthel, Thomas (1961), Erwiderung, *Zeitschrift für Ethnologie*, 86: 279–281.

Basedow, Herbert (1927), Subincision and kindred rites of the Australian aboriginal, *Journal of the Royal Anthropological Institute of Great Britain and Ireland*, 57: 123–156.

Battaglia, R. (1927), Review of Sergi, Giuseppe, *Le prime e le piu antiche civilta*, *Anthropos*, 22: 314–316.

Baumann, Hermann (1928), The division of work according to sex in African hoe culture, *Africa*, 1: 289–319.

— (1944a), Zur Morphologie des afrikanischen Ackergerätes, *Wiener Beiträge zur Kulturgeschichte und Linguistik*, 6: 192–322.

— (1944b), Review of *Acta Tropica*, *Wiener Beiträge zur Kulturgeschichte und Linguistik*, 6: 348–351.

— (1948), *Les peuples et les civilisations de l'Afrique*. Paris, Payot.

— (1955), *Das doppelte Geschlecht*. Berlin, Dietrich Reimer.

Beasley, Ruth (1973), Letter of June 6 (Institute for Sex Research, founded by Alfred C. Kinsey; Miss Beasley is the Coordinator, Information Service).

Beitl, Klaus, Ed. (1972), *Volkskunde. Fakten und Analysen. Festgabe für Leopold Schmidt zum 60. Geburtstag*. Sonderschriften des Vereins für Volkskunde in Wien, vol. 2. Wien, Selbstverlag des Vereines für Volkskunde.

Beitl, Richard (1955), *Wörterbuch der deutschen Volkskunde*. Stuttgart, Alfred Kroener.

Bentzien, Ulrich (1972), Der Mecklenburgische Haken im 19. Jahrhundert: Über die Renaissance eines älteren Pfluggerätes. In Balassa, 1972. Pp. 487–499.

Berch, Anders (1795), Anmerkungen über die schwedischen Pflüge. *Abhandlungen der kgl. schwedischen Akademie der Wissenschaften auf das Jahr 1759*, 21: 192–214.

— (1773), Methodus investigandi origines gentium, ope instrumentorum ruralium, *Nova acta regiae societatis scientiarum Upsaliensis*, 1: 32–37.

— (1953), Methodus investigandi origines gentium, ope instrumentorum ruralium. Swedish translation by Gösta Claesson, edited and with an introduction by Ragnar Jirlow. *Lychnos, Lärdomshistoriska samfundets årsbok. Annual of the Swedish History of Science Society*. Pp. 95–100.

Berg, Gösta (1928), Den Svenska sädesharpan och den kinesiska. In *Nordiskt folkminne, Studier tillägnade C. W. von Sydow*. Pp. 19–24.

— (1961), Die Erforschung der schwedischen Volkskultur: Ein historischer Rückblick. In *Schwedische Volkskunde: Quellen, Forschung, Ergebnisse. Festschrift für Sigfrid Svensson*. Stockholm, Almquist & Wiksell.

— (1971), Kopftragen in Schweden. In: Szabadfalvi and Újváry, 1971, pp. 381–394.

Bergh, G. C. J. J. van den (1971), Volksgerichten in Limburg. In: *Limburg, Bijdragen tot de taal en het historisch volksleven van de beide Limburgen en het aangrenzende Rijnland,* pp. 85–96.

Bericht (1968), *Internationale Arbeitskonferenz über die ethnologische Kartographie,* 8–10. Februar 1966. Zagreb, Etnološki Atlas Jugoslavije.

Berndt, Ronald M. (1951), *Kunapipi.* Melbourne, F. W. Cheshire (also New York, International Universities Press).

— and Catherine H. Berndt (1951), *Sexual Behavior in Western Arnhem Land.* Viking Fund Publications in Anthropology 16. New York, The Viking Fund.

— (1964) *The world of the first Australians.* Sydney, Ure Smith.

Bernheim, Ernst (1908), *Lehrbuch der historischen Methode und der Geschichtsphilosophie.* Sixth edition. Leipzig, Duncker & Humblot.

Besch, Werner (1972), Sprachnorm-Kompetenz der Bundestages? Das Beispiel der Handwerkernamen. In *Festschrift Matthias Zender:* 993–1015.

Bettelheim, Bruno (1954), *Symbolic wounds.* Glencoe, The Free Press.

Bianchi, Ugo (1965), *Storia dell'Etnologia.* Rome, Edigioni Abete.

Bloomfield, Leonard (1933), *Language.* New York, Henry Holt and Company.

Blüher, Hans (1953), *Werke und Tage: Geschichte eines Denkers.* Munich, Paul List Verlag.

Boas, Franz (1940), *Race, language and culture.* New York, Macmillan.

Bock, Philip K. (1967), Love Magic, Menstrual Taboos, and the Facts of Geography, *American Anthropologist,* 69: 213–217.

Bornemann, Eduard (1973), *Griechische Grammatik, unter Mitwirkung von Ernst Risch.* Frankfurt am Main, Moritz Diesterweg.

Bratanić, Branimir (1951), On the problem of immigration of the southern Slavs to South-Eastern Europe: Some ethnographic and lexical facts. *Zbornik radova Filozofskog fakulteta Sveučilišta u Zagrebu,* I: 221–250.

— (1955), Einige Möglichkeiten zur Fortführung der Pfluggeräteforschung. *Actes du IV^e Congrès International des Sciences Anthropologiques et Ethnologiques,* Vienne, 1–8 Septembre 1952, vol. 2: 90–98. Vienna, Adolf Holzhausens Nachfolger.

— (1958), Review of Haudricourt and Jean-Brunhes Delamarre, L'Homme et la charrue, *Anthropos,* 53: 1041–1046.

— (1960), Some similarities between ards of the Balkans, Scandinavia, and Anterior Asia, and their methodological significance. In *Selected Papers of the Fifth International Congress of Anthropological and Ethnological Sciences, Philadelphia, September 1–9, 1956. Men and Cultures.* Edited under the Chairmanship of Anthony F. C. Wallace. Philadelphia 1960, pp. 221–228.

— (1961), Einige unbekannte Zusammenhänge zwischen den Ostalpen und dem serbisch-mazedonisch-bulgarischen Grenzgebiet. In *Protokollmanuskript, 2. Arbeitstagung über Fragen des Atlas der deutschen Volkskunde.* Pp. 63–65.

— (1972), Ethnologische Kartographie und ethnologische Atlanten. In Fellenberg, 1972. Pp. 4–9 (also 9–13).

Brew, J. O., Ed. (1968), *One hundred years of anthropology.* Cambridge, Mass., Harvard University Press.

Bringéus, Nils-Arvid (1962–63), Swedish ethnology before 1900, *Folk-Liv,* 26–27: 20–39.

— (1971), Probleme und Methoden ethnologischer Nahrungsforschung im Lichte jüngster schwedischer Untersuchungen. *Ethnologia Scandinavica, A Journal for Nordic Ethnology,* 1: 19–36.

— (1972), Die Einführung der Weihnachtskrippe in schwedischen Kirchen. In *Festschrift Matthias Zender.* Pp. 182–194.

Broce, Gerald (1973), *History of anthropology.* Minneapolis, Burgess Publishing Company.

Brockhaus Enzyklopädie (1970), 17th edition, vol. 10, Wiesbaden.

Bryk, Felix (1928), Einige „Parallelen" zum Alten Testament aus Kavirondo (Kenya Colony. Ostafrika.). *Völkerkunde,* 4: 104–108.

— (1931), *Die Beschneidung bei Mann und Weib: Ihre Geschichte, Psychologie und Ethnologie.* Neubrandenburg, Gustav Feller.

Burcaw, George Ellis (1973), *The Saxon house: A cultural index in European ethnography.* Moscow, Idaho, University of Idaho Museum.

Burgstaller, Ernst (1972), Zur kulturgeographischen Brückenstellung des Zentralraumes von Oberösterreich. In *Festschrift Matthias Zender.* Pp. 106–122.

Burgstaller, Ernst and Helbok, Adolf, Eds. (1959), *Österreichischer Volkskundeatlas.* Linz an der Donau and Graz–Köln, Hermann Böhlaus Nachf.

Calverton, V. F., Ed. (1931), *The making of man: An outline of anthropology.* New York, The Modern Library, Random House.

Campbell, Joseph (1968), *The hero with a thousand faces.* Bollingen series 17. Second edition, second printing. Princeton University Press.

Campbell, Meredith F. and Harrison, J. Hartwell, Eds. (1970), *Urology,* vol. 2., third edition. Philadelphia, W. B. Saunders Co.

A Catholic commentary on Holy Scripture (1952), London, Thomas Nelson and Sons.

Cawte, J. E. (1968), Further comment on the Australian subincision ceremony, *American Anthropologist,* 70: 961–964.

— Djagamara, Nari, and Barrett, M. G. (1966), The meaning of subincision of the urethra to aboriginal Australians, *British Journal of Medical Psychology,* 39: 245–253.

Chen, You C., and Woolley, Jr., Paul V. (1971), Genetic Studies on hypospadias in males. *Journal of Medical Genetics* 8: 153–159.

Chung, C. S., and Myrianthopoulos, N. C. (1968), Racial and prenatal factors in major congenital malformations, *American Journal of Human Genetics,* 20: 44–60.

Collins, David (1804), *An account of the English colony in New South Wales.* London.

Cox, H. L. (1971a), Limburg: Bijdragen tot de taal en het historisch volksleven van de beide Limburgen en het aangrenzende Rijnland. *Neerlands Volksleven,* XXI (1), Wassenaar.

— (1971b), Het haalleiden of halen, Relictvormen van een oude inwijdingsrite in Nederlands- en Belgisch-Limburg. In Cox, 1971a, pp. 60–84.

— (1972a), Martelganz/Mateljoos. Zur Herkunft und Bedeutung einer mundartlichen Bezeichnung des Erntefestes im deutsch–niederländischen Grenzland. In *Festschrift Matthias Zender.* Pp. 174–181.

— (1972b), Elektronische Datenverarbeitung in der thematischen Kartographie: Kartierungstechnik in Ethnologie und Ethnolinguistik, *Ethnologia Europaea,* 6: 108–127.

Darwin, Charles (1878), *The descent of man.* New edition, revised and augmented. New York, D. Appleton and Company.

Delamarre, Mariel Jean-Brunhes (1969), *Géographie et ethnologie de l'attelage au joug en France du XVIIe siècle à nos jours.* Brno, Uherské Hradiště.

Demeusy, Anne (1973), Close Museum, court is asked. *The Hartford Courant,* April 11.

Demtschenko, N. A. (1972), Die moldauischen landwirtschaftlichen Geräte im 18. bis zum beginnenden 20. Jahrhundert. In Balassa, 1972a, pp. 243–277.

Diamond, Stanley (1960), *Culture in history: Essays in honor of Paul Radin.* New York, Columbia University Press.

Dictionnaire complet Français–Allemand–Anglais à l'usage des trois nations (1883), 12th edition. Leipzig, F. A. Brockhaus.

Dietz, Josef (1972), St. Helena in der rheinischen Überlieferung. In *Festschrift Matthias Zender.* Pp. 356–383.

Driver, Harold E. (1962), The contribution of A. L. Kroeber to culture area theory and practice. Supplement to *International Journal of American Linguistics,* 28 (2).

Driver, Harold E., and William C. Massey (1957), Comparative studies of North American Indians. *Transactions of the American Philosophical Society,* New Series, 47 (part 2).

Drost, Dietrich (1967), Töpferei in Afrika: Technologie. *Veröffentlichungen des Museums für Völkerkunde zu Leipzig,* 15. Berlin, Akademie-Verlag.

— (1968), Töpferei in Afrika: Ökonomie und Soziologie. *Jahrbuch des Museums für Völkerkunde zu Leipzig,* 25: 131–270.

Du Bois, Cora (1963), The curriculum in cultural anthropology. In David G. Mandelbaum, Gabriel Lasker and Ethel M. Albert (Eds.), *The teaching of anthropology.* American Anthropological Association, Memoir 94.

Eames, Edwin, and Howard Robboy (1974), The socio-cultural context of an Italian–American dietary item. In Jorgensen and Truzzi. Pp. 505–516.

Edgerton, Franklin (1966), Sir William Jones. In Sebeok, 1966. Pp. 1–18.

Eggan, Fred (1968), One hundred years of ethnology and social anthropology. In Brew, 1968. Pp. 116–149.

Ellsworth, Lucius F. and O'Brien, Maureen A. Eds. (1969), *Material culture: Historical agencies and the historian.* Philadelphia, Book Reprint Service.

Encyclopaedia Judaica, (1971), vol. 5. Macmillan.

Encyclopaedia of religion and ethics (1911), *see* Louis H. Gray.

Ennen, Edith (1972), *see Festschrift Matthias Zender.*

Erixon, Sigurd (1937), Die Frage der kartographischen Darstellung, vom Standpunkt der nordischen Ethnologie in Schweden aus betrachtet, *Folk,* I: 168–180.

— (1938), *Svenskt Folkliv.* Uppsala, J. A. Lindblads Förlag.

— (1948), Svenska årder. *Liv och folkkultur,* 1: 129–157.

— (1957), *Atlas över Svensk folkkultur, utgiven av Kungl. Gustav Adolfs Akademien.* Utgiven under redaktion av Sigurd Erixon. Uddevalla, Bokförlaget Niloé.

— (1960–61), De nationella atlasverken över folkkultur, *Folk-Liv,* 24–25: 90–111.

— (1962–63), Folklivsforskningens framväxt, *Folk-Liv,* 26–27: 64–102.

— (1964–65), Folklivsforskningens framväxt, *Folk-Liv,* 28–29: 7–63.

Etnološki atlas Jugoslavije (1963), *Pokusne Karte.* Zagreb.

Fahringer, Fritz (1971), Das Säschaff: Ein Beitrag zur Verwendung dieses Sägerätes im Ausseerland, *Blätter für Heimatkunde,* 45: 55–58.

Fellenberg, J., gen. Reinold (1970), *Zweite Arbeitskonferenz der Organisationskommission für den Volkskundeatlas Europas und seiner Nachbarländer.* Bericht. Bonn, Atlas der deutschen Volkskunde.

— (1972), *Dritte Arbeitskonferenz der Organisationskommission für den Volkskundeatlas Europas und seiner Nachbarländer.* 11–14. Mai 1970 in Helsinki. Bericht, herausgegeben im Auftrag der Ständigen Internationalen Arbeitskommission. Bonn 1972.

Fenton, Alexander (1971), The place of oatmeal in the diet of Scottish farm servants in the eighteenth- and nineteenth-centuries. *Szabadfalvy* and *Újváry,* 1971: 87–101.

— (1973), The scope of regional ethnology. *Folk Life,* 11.

Ferrner, Bengt (1776), *Åminnelse-Tal, öfver Första Juris, Öconom, & Commercior. Professoren ... Herr Doctor Anders Berch, Hållet ... för Kongl. Vetenskaps-Academien.* Stockholm, Joh. Georg Lange.

Festschrift Matthias Zender: Studien zu Volkskultur, Sprache und Landesgeschichte, (1972). Edith Ennen and Günter Wiegelmann, (Eds.) („In Verbindung mit Branimir Bratanić, Jorge Dias, Henri Draye, Gerda Grober-Glück, Franz Petri Rudolf Schützeichel"). Bonn, Ludwig Röhrscheid.

Fourth international directory of anthropologists (1967), *Current Anthropology,* 8: 549–646.

Frantz, Ch. (1967), *Guide to graduate departments of anthropology for the year 1967–68 and list of members.* Washington, D. C., American Anthropological Association.

Frauenknecht, Gertrud (1972), Die evangelische Pfarrkirche Veitsbronn als Ziel einer Wallfahrt. In *Festschrift Matthias Zender.* Pp. 384–394.

Frobenius, Leo (1897), Der westafrikanische Kulturkreis. *Petermanns Geographische Mitteilungen,* 43: 225–236; 262–267; 44: 193–204; 265–271.

— (1898a), *Der Ursprung der afrikanischen Kulturen.* Berlin, Bornträger.

— (1898b), Der Ursprung der afrikanischen Kulturen. *Zeitschrift der Gesellschaft für Erdkunde zu Berlin,* 33: 111–25.

— (1900), *The origin of African civilizations.* Smithsonian Report for 1898, Washington. Pp. 637–50.

— (1907), Ethnologische Ergebnisse der ersten Reisen der Deutschen Inner-Afrikanischen Forschungs-Expedition, *Zeitschrift für Ethnologie,* 39: 311–333.

— (1922–30), *Atlas Africanus. Belege zur Morphologie der afrikanischen Kulturen.* Munich, C. H. Beck. Berlin, W. de Gruyter.

— (1925), *Erlebte Erdteile,* vol. 1, *Ausfahrt.* Frankfurt am Main, Frankfurter Societäts-Druckerei.

— (1931), *Morphologie des afrikanischen Bogengerätes.* Berlin, W. de Gruyter.

— (1932), *Morphology of the African Bow-Weapon.* Berlin, W. de Gruyter.

— (1933), *Kulturgeschichte Afrikas.* Zürich, Phaidon Verlag.

Fröhlich, Willy, Bolz, Ingeborg and Stöhr, Waldemar (1958), Ethnologische Zeitschriften und Publikationsreihen. In Leonhard Adam und Hermann Trimborn, *Lehrbuch der Völkerkunde,* pp. 286–292, Stuttgart, Ferdinand Enke.

Fromkin, Victoria, and Rodman, Robert (1974), *An introduction to language.* New York, Holt, Rinehart.

Füzes, Endre (1972), Die Getreidespeicher im südlichen Teil des Karpatenbeckens. In Balassa, 1972a. Pp. 583–619.

Gavazzi, Milovan (1971), Ein Beitrag zur Kenntnis der balkan-karpatischen Kulturströmungen. In Szabadfalvi and Újváry, 1971. Pp. 213–222.

Geiger, Paul and Weiss, Richard, Eds. (1950), *Einführung in den Atlas der schweizerischen Volkskunde.* Erlenbach–Zürich, Eugen Rentsch.

Gerschenkron, Alexander (1971), The concept of continuity in German anthropology, *Comparative studies in society and history. An International Quarterly,* 13 (3): 351–357.

Gleason, H. A., Jr. (1955), *An Introduction to descriptive linguistics.* New York, Henry Holt.

Goossens, J. (1971), Bijdrage tot de woordstratigrafie van de Limburgia Romana. In: Cox, 1971a. Pp. 28–38.

Grabner, Elfriede (1972), Maria vom guten Rat. In Beitl, 1972. Pp. 327–338.

Graebner, Fritz (1905), Kulturkreise und Kulturschichten in Ozeanien, *Zeitschrift für Ethnologie,* 37: 28–53.

— (1906), Wanderung und Entwicklung sozialer Systeme in Australien, *Globus,* 90: 181–86, 207–19, 220–24, 237–41.

— (1908a), Die sozialen Systeme in der Südsee. Erster Artikel, *Zeitschrift für Sozialwissenschaft,* 11: 663–681.

— (1908b), Review of *Die Eingeborenen der Kolonie Südaustralien,* by Erhardt Eylmann, *Zeitschrift für Ethnologie,* 40: 1005–1006.

— (1909a), Völkerkunde der Santa-Cruz-Inseln, *Ethnologica,* 1: 71–184.

— (1909b), Die melanesische Bogenkultur und ihre Verwandten, *Anthropos,* 4: 726–780; 990–1032.

— (1909c), Zur australischen Religionsgeschichte. Eine kritisch-methodische Studie, *Globus,* 96: 341–344; 362–366; 373–378.

— (1911a), *Methode der Ethnologie.* Heidelberg, Winters Universitätsbuchhandlung.

— (1911b), Völkerkunde und Geschichte, *Preussische Jahrbücher,* Oktober 1911: 83–92.

— (1919–1920), Thor und Maui, *Anthropos,* 14–15: 1101–1119.

— (1923), Ethnologie. *Die Kultur der Gegenwart, Dritter Teil, Fünfte Abteilung.* Leipzig, B. G. Teubner. Pp. 435–587.

— (1925), Gentilorganisation in der Bibel. *Sitzungsberichte der Anthropologischen Gesellschaft in Wien, Jahrgang 1924–25,* Vienna. P. [15].

— (1927), Adolf Bastians 100. Geburtstag, *Ethnologica* 3: IX–XII.

— (1931), Causality and culture. In V. F. Calverton (Ed.), *The making of man,* New York, The Modern Library. Pp. 421–428.

Granlund, John (1968), Skånsk senap och senapsknäcka, *Gastronomisk Kalender,* 1968: 20–51.

— (1971), Pfingstbraut und Pfingsthochzeit: Versuch zur Analyse eines volkstümlichen Frühlingsbrauches. In Szabadfalvi and Újváry, pp. 235–270.

Grau, Rudolf (1955), Geschlechtsorientierte Polaritäten in Negerafrika. In *Plischke-Festschrift,* pp. 161–175.

Gray, Louis H. (1911), Circumcision (Introductory). In James Hastings (Ed.), *Encyclopedia of religion and ethics.* Vol. 3. Pp. 659–670.

Greenough, James B., and Kittredge, George L. (1968), Folk-etymology. In Donald E. Hayden, E. Paul Alworth and Gary Tate (Eds.), *Classics in linguistics*. London, Peter Owen. Pp. 144–154.

Grober-Glück, Gerda (1972), Lokalnamen in deutschsprachigen Bezeichnungen von Sagengestalten. Verbreitung und Deutung. In *Festschrift Matthias Zender*. Pp. 144–168.

Gunda, Béla (1971), *Gunda Festschrift: See* Szabadfalvy and Újváry.

Guptill, Roger S. (1927), *A comparison of the religion of the Bantu with that of the Old Testament as a Preparation for Christianity*. Master of Arts Thesis, The Kennedy School of Missions, The Hartford Seminary Foundation, Hartford, Connecticut.

Haddon, Alfred C. (1934), *History of anthropology*. The Thinker's Library, No. 42. London, Watts & Co.

Hahn, Eduard (1909), Review of K. Rhamm, Ethnographische Beiträge zur Germanisch-slavischen Altertumskunde. *Zeitschrift für Ethnologie*, 41, pp. 586–588, and 975.

Halpern, Joel M. (1970–71), Ethnology in Yugoslavia, *East European Quarterly* 4.

Halpern, Joel M. and Hammel, Eugene A. (1967a), Ethnology in Yugoslavia. *Program, American Anthropological Association, 66th Annual Meeting*. Washington, D. C. P., 22.

— (1967b), Ethnology in Yugoslavia. *Abstracts. American Anthropological Association, 66th Annual Meeting*, Washington, D. C., P. 34.

— (1969), Observation on the Intellectual History of Ethnology and other Social Sciences in Yugoslavia, *Comparative Studies in Society and History*, 11: 17–26.

Hamp, Eric P. (1973), Letter to the Editor. In *Current Anthropology* 14:333.

Hanisch, Konrad (1972), Ethnographische Atlasvorhaben in Polen. In *Festschrift Matthias Zender*. Pp. 296–308.

Harris, Marvin (1968), *The rise of anthropological theory: A history of theories of culture*. New York, Thomas Y. Crowell Company.

Hays, H. R. (1958), *From ape to angel: An informal history of social anthropology*. New York, Alfred A. Knopf.

Heine-Geldern, Robert (1964), One hundred years of ethnological theory in the German-speaking countries: Some milestones, *Current Anthropology*, 5: 407–418.

Herskovits, Melville J. (1951), *Man and his works*. Fifth Printing. New York, Alfred A. Knopf. (First published in 1948.)

— (1953), *Franz Boas: The science of man in the making*. New York, Charles Scribner's Sons.

Herskovits, Melville J. and Ames, Barbara (1950), *International directory of anthropologists*. Third Edition. Washington, D. C., National Research Council.

Himes, Norman E. (1936), *Medical History of Contraception.* Baltimore, Williams & Wilkins.

— (n. d.) *Medical History of contraception.* With a new preface by Christopher Tietze. New York, Schocken Books.

Hockett, C. F. (1958), *A course in modern linguistics.* New York, Macmillan.

Hodgen, Margaret T. (1950), Similarities and dated distributions, *American Anthropologist,* 52: 445–467.

Hoffmann, A. G. (1822), Beschneidung. In J. S. Ersch und J. G. Gruber, *Allgemeine Encyclopädie der Wissenschaften und Künste in alphabetischer Folge ... Neunter Teil, Bene bis Bibeh.* Leipzig, Johann Friedrich Gleditsch.

Hogbin, H. Ian (1934), *Law and order in Polynesia: A study of primitive legal institutions.* New York, Harcourt Brace and Company.

Höltker, Georg (1968), Ältere Bilddokumente zur Narbentatauierung in Neuguinea, *Jahrbuch des Museums für Völkerkunde zu Leipzig,* 25: 7–28.

Hommel, Fritz (1910), Vorwort. In M. Merker, *Die Masai.* Berlin, Dietrich Reimer (Ernst Vohsen). Pp. XIII–XXIII.

Horton, Charles E. and Devine, Jr., Charles J. (1972), Hypospadias and epispadias, *Clinical Symposia* (Ciba) 24, 3: 2–27.

Hultkrantz, Åke (1960), *General ethnological concepts.* Copenhagen, Rosenkilde and Bagger.

— (1973), *Metodvägar inom den jämforande religionsforskningen.* Stockholm, Esselte Studium.

Hvarfner, Harald (1968–69), Die Verbreitung und Stabilität der Arbeitsmethoden: Einige Überlegungen zur ethnologischen Kartografie Europas, *Ethnologia Europaea,* 203: 191–196.

Index 1 (1949), 1906–44. *Index auctorum. Articuli Analecta Additamenta.* Posieux (Fribourg), Redaktion des Anthropos.

The Interpreter's Bible (1953). New York and Nashville, Abingdon–Cokesbury Press.

Isaac, Erich (1964a), Circumcision as a Covenant Rite, *Anthropos,* 59: 444–456.

— (1964b), Relations between the Hebrew Bible and Africa, *Jewish Social Studies,* 26: 87–98.

Jaberg, K., and Jud, J. (1943), *Sprach- und Sachatlas Italiens und der Südschweiz. Illustrationsband: Paul Scheuermeier, Bauernwerk.* Erlenbach–Zürich, Eugen Rentsch.

Jansson, Sam Owen (1961), Kulturgrenzen und Kulturwege. In *Schwedische Volkskunde, Festschrift für Sigfrid Svensson,* Stockholm, Almqvist & Wiksell. Pp. 108–130.

Jean-Brunhes Delamarre, Mariel (1969), *see* Delamarre, Mariel Jean-Brunhes.

Jensen, Adolf Ellegard (1953), Dual-Systeme in Nordost-Afrika, *Anthropos,* 48: 737–759.

— (1960a), Beziehungen zwischen dem Alten Testament und der nilotischen Kultur in Africa. In Stanley Diamond (Ed.), *Culture in history*. Columbia University Press, New York. Pp. 449–466.

— (1960b), Methoden und Ziele der Ethnologie. In: Burghard Freudenfeld (Ed.), Völkerkunde, Verlag C. H. Beck, Munich. Pp. 1–14.

The Jerome biblical commentary (1968), vol. 1, *The Old Testament*. Englewood Cliffs, New Jersey, Prentice Hall.

Jirlow, Ragnar (1936), Bidrag till några europeiska jordbruksredskaps historia. *Kungl. landtbruksakademiens handlingar och tidskrift*, 1936: 267–304.

— (1953), Anders Berch, Metod att utröna folkens ursprung med hjälp av deras jordbruksredskap. Utgiven med en inledning. *Lychnos, Lärdomshistoriska samfundets årsbok, Annual of the Swedish History of Science Society*, 1953: 92–100.

— (1970), Die Geschichte des schwedischen Pfluges. *Nordiska Museets Handlingar*, 72. Stockholm, Boktryckeri AB Thule.

Johnston, Sir Harry H. (1886), *The Kilima-Njaro expedition: A record of scientific exploration in eastern equatorial Africa* ... London, Kegan Paul, Trench & Co.

— (1902), *The Uganda protectorate*. Vol. 2. London, Hutchinson & Co.

Jones, G. I. (1952), Africana Upsaliensia, *Nature*, 170 (4337): 993.

Jong, W. de (1971), Besnijdenis als orozakelijke factor van peniskanker, *Nederlandsch Tijdschrift voor Geneeskunde*. 115, (41): 1709–1710.

Jorgensen, Joseph G., and Truzzi, Marcello (1974), *Anthropology and American life*. Englewood Cliffs, N. J., Prentice-Hall.

Karasek, Alfred, and Lanz, Josef (1974), *Krippenkunst in Böhmen und Mähren vom Frühbarock bis zur Gegenwart*. Marburg, N. G. Elwert Verlag.

Karlinger, Felix (1972), Die Volkserzählungen Mallorcas. Versuch einer kartographischen Erfassung. In Beitl, 1972. Pp. 465–469.

Karsch-Haack, F. (1911), *Das gleichgeschlechtliche Leben der Naturvölker*. Munich, Ernst Reinhardt Verlag.

Kauffmann, Hans E. (1968), Das Fadenkreuz, sein Zweck und seine Bedeutung (II). *Ethnologica*, new series, 4: 264–313.

Kinsey Institute (1973), *see* Beasley.

Kirn, Paul (1963), *Einführung in die Geschichtswissenschaft*, 4th ed. Berlin, Walter de Gruyter.

Kisbán, Eszter (1972), Maisnahrung im Karpatenbecken. In *Festschrift Matthias Zender*. Pp. 264–280.

Klaatsch, Hermann (1907). Some notes on scientific travel amongst the black population of tropical Australia in 1904, 1905, 1906. *Report of the Eleventh Meeting of the Australasian Association for the Advancement of Science*, held at Adelaide, 1907: 577–592.

Kłodnicki, Zygmunt (1971), Transport mit Menschenkraft in Polen in der ersten Hälfte des XX. Jahrhunderts. In Szabadfalvi and Újváry, 1971. Pp. 589–606.

Kluckhohn, Clyde (1936), Some reflections on the method and theory of the Kulturkreislehre. *American Anthropologist,* N. S., 38: 157–196.

Koppers, Wilhelm (1927a), Review of Schroeter, *Der Mythus von Orient und Okzident, Anthropos,* 22: 311–313.

— (1927b), Fr. Krause's „Strukturlehre" als Teil der kulturhistorischen Methode, *Anthropos,* 22: 614–617.

— (1928), Natur- und Kulturgeschichte des Menschen in ihren gegenseitigen Beziehungen, *Anthropos,* 23: 685–690.

— (1959), Grundsätzliches zur ethnologischen Kulturkreislehre. In Emil Breitinger *et al.* (Eds.), *Beiträge Österreichs zur Erforschung der Vergangenheit und Kulturgeschichte der Menschheit.* Horn, Niederösterreich, Ferdinand Berger. Pp. 110–126.

Koren, Hanns (1972), Die steirischen Eggen. In Beitl, 1972. Pp. 61–98.

Kothe, Heinz (1957–58), Erntemesser und Sichel in ihrer Bedeutung für die Frühgeschichte der Landwirtschaft, *Folk-Liv,* 21–22: 61–78.

Kramer, Fritz L. (1966), *Breaking Ground: Notes on the distributions of some simple tillage tools.* Sacramento, California, Sacramento Anthropological Society.

Kraus, Hans-Joachim (1966), *Worship in Israel: A cultic history of the Old Testament.* Richmond, Virginia, John Knox Press.

Kroeber, A. L. (1925), *Handbook of the Indians of California.* Smithsonian Institution, Bureau of American Ethnology, Bulletin 78. Washington, Government Printing Office.

— (1941), *Culture Element Distributions: XV. Salt, Dogs, Tobacco.* Anthropological Records, 6 (1). Berkeley and Los Angeles. University of California Press.

— (1947), *Cultural and natural areas of Native North America.* Berkeley and Los Angeles. University of California Press.

— (1948), *Anthropology,* New Edition, Revised 1948, New York, Harcourt, Brace and Company.

Kroll, Hubert (1928), Die Haustiere der Bantu. *Zeitschrift für Ethnologie,* 60: 177–290.

Kurath, Hans and McDavid, Jr., Raven I. (1961), *The pronunciation of English in the Atlantic states.* Ann Arbor, University of Michigan Press.

Lagercrantz, Sture (1935), Fingerverstümmelungen und ihre Ausbreitung in Afrika, *Zeitschrift für Ethnologie,* 67: 129–157.

— (1937), Ringdolche, Armdolche und Schlagringe in Afrika, *Zeitschrift für Ethnologie,* 69: 389–443.

— (1938a), *Beiträge zur Kulturgeschichte der afrikanischen Fallen.* Stockholm, Bokförlags Aktiebolaget Thule.
— (1938b), Zur Verbreitung der Monorchie, *Zeitschrift für Ethnologie,* 70: 199–206.
— (1939a), *A contribution to the study of anomalous dentition and its ritual significance in Africa.* Statens Ethnografiska Museum, Smärre Meddelanden 16. Stockholm.
— (1939b), Zur Verbreitung des Klystiers in Negerafrika. In M. Hesch and G. Spannaus (Eds.), *Kultur und Rasse, Otto Reche Festschrift.* Munich, J. F. Lehmann.
— (1950), *Contribution to the ethnography of Africa.* London, Kegan Paul, Trench, Trubner & Co.
— (1954), *African methods of fire-making. Studia Ethnographica Upsaliensia,* X. Almqvist & Wiksell.
— (1968), African Tally-Strings, *Anthropos,* 63: 115–128.
— (1973), Counting by means of tally sticks or cuts on the body in Africa, *Anthropos,* 68: 569–588.
Lehmann, F. R. (1957), Towards a new explanation of circumcision. *Sociologus,* N. S. 7: 57–74.
Lehmann, Johannes (1904), Die Pfahlbauten der Gegenwart, ihre Verbreitung und genetische Entwicklung, *Mitteilungen der Anthropologischen Gesellschaft in Wien,* 34: 19–51.
Leiner, Maria (1972), Das Getreideausschlagen in der Steiermark. In Beitl, 1972. Pp. 105–119.
Lenz, Fritz (1921), *Menschliche Auslese und Rassenhygiene.* (Vol. II of Baur, Erwin, and Eugen Fischer, *Grundriss der menschlichen Erblichkeitslehre und Rassenhygiene*). Munich, J. F. Lehmann.
Leser, Paul (1928), Westöstliche Landwirtschaft: Kulturbeziehungen zwischen Europa, dem vorderen Orient und dem Fernen Osten. In W. Koppers (Ed.), *P. W. Schmidt-Festschrift.* Wien, Anthropos Administration. Pp. 416–484.
— (1931), *Entstehung und Verbreitung des Pfluges.* Münster, Aschendorff.
— (1952), Review of: *Notes and Queries in Anthropology, International Review of Missions,* 41: 535–38.
— (1954), Review of E. Cecil Curwen and Gudmund Hatt, *Plough and Pasture, Anthropos,* 49: 729–736.
— (1960a), Review of: George Peter Murdock, *Africa. Technology and Culture,* 1: 256–59.
— (1960b), Felder und Bodenbaugeräte der Nyakyusa. *Ethnologica,* New Series, 2: 363–383.
— (1961), Review of: Branimir Bratanić, Some similarities between ards of the Balkans, Scandinavia, and Anterior Asia, and their methodological significance. *Deutsches Jahrbuch für Volkskunde,* 7: 360–362.

— (1962a), Review of: Burghard Freudenfeld (Ed.): *Völkerkunde: Zwölf Vorträge zur Einführung in ihre Probleme. American Anthropologist,* 64: 855–857.
— (1962b), Review of *Beiträge zur Volkskunde ... Journal of American Folklore,* 75 (298): 357–358.
— (1963), Zur Geschichte des Wortes Kulturkreis, *Anthropos,* 58: 1–36; 565.
— (1964), Comment on Heine-Geldern, *Current Anthropology,* 5: 416–418.
— (1965), European vs. American Anthropology, *Current Anthropology,* 6: 311–313; 317–318.
— (1967), The academic position of European ethnology in North America, *Ethnologia Europaea,* 1: 320–322.
— (1968), Dreschen und Worfeln in der Grossen Kabylei: Kulturwandel und Kulturstillstand in Nordafrika, *Anthropos,* 63: 201–211.
— (1970), Die Bedeutung ethnologischer Atlanten für die Geschichtswissenschaft. In Fellenberg, 1970. Pp. 17–22.
— (1970–71), Ethnology in Germany, *East European Quarterly,* 4: 275–292.
— (1972a), Die Bedeutung der Erforschung der materiellen Kultur für die Ethnologie, *Ethnologia Europaea,* 6: 196–202.
— (1972b), Probleme kulturräumlicher Zusammenhänge. In Fellenberg, 1972. Pp. 53–54. *Listed here only in order to state that neither the title nor the text are by Paul Leser.*
Lindblom, Gerhard (1926), Einige „Parallelen" zum Alten Testament aus Kavirondo (Kenya Colony, Ostafrika), *Völkerkunde* 2: 158–161.
— (1927a), *The use of stilts especially in Africa and America.* Stockholm, Riksmuseets ethnografiska avdelning.
— (1927b), *Fighting bracelets and kindred weapons in Afrika.* Stockholm, Riksmuseets etnografiska avdelning.
— (1928), *The spiked wheel-trap and its distribution.* Stockholm, Riksmuseets etnografiska avdelning.
— (1933), *Two kinds of fishing implements: (1) The plunge basket (Stülpkorb) in Africa and elsewhere. (2) The circular cast-net in Africa.* By T. Leth and K. G. Lindblom. Stockholm, Riksmuseets etnografiska avdelning.
— (1939), *Wire-drawing, especially in Africa.* Stockholm, Riksmuseets etnografiska avdelning.
— (1949), *The one-leg resting position (Nilotenstellung), in Africa and elsewhere.* Stockholm, Riksmuseets etnografiska avdelning.
Lowie, Robert H. (1937), *The history of ethnological theory.* New York, Rinehart & Company, Inc.
Malinowski, Bronisław (1934), Preface. In Hogbin, 1934.
Manndorf, Hans (1952), Die Lokalinterpretation: Eine grundsätzliche Studie zur Methode ... *Veröffentlichungen der urgeschichtlichen Arbeitsgemeinschaft,* vol. 1. Vienna, Austria. Pp. 139–149.

Marschall, Wolfgang (1968), Metallurgie und frühe Besiedlungsgeschichte Indonesiens, *Ethnologica,* new series 4: 29–263.

Marzan, J. de (1910), Mutilatio ethnica in Australia subincisio (mika) dicta existitne in insula Fiji? *Anthropos,* 5: 808–809.

Meinhof, Carl (1904), Über M. Merker's „Masai". *Zeitschrift für Ethnologie,* 36: 735–744.

Mercier, P. (1966), *Histoire de l'anthropologie.* Paris, Presses Universitaires de France.

Merker, Moritz (1910), *Die Masai. Ethnographische Monographie eines ostafrikanischen Semitenvolkes. Zweite verbesserte und vermehrte Auflage.* Berlin, Dietrich Reimer (Ernst Vohsen).

Moltschanowa, L. A. (1972), Die landwirtschaftlichen Geräte der belorussischen Bauernschaft im 19. Jahrhundert. In Balassa, 1972a. Pp. 205–241.

Montagu, Ashley (1937), *Coming into being among the Australian aborigines.* London, George Routledge & Sons, Ltd.

— (1937–38), The origin of subincision in Australia, *Oceania,* 8: 193–207.

— (1969), Circumcision. *Encyclopedia Britannica,* vol. 5. Chicago, William Benton. Pp. 799–800.

Moss, Leonhard W. (1974), Study group for Italianist scholars, *Newsletter of the American Anthropological Association,* 15 (2): 10.

Mühlmann, Wilhelm (1938), Geschichtliche Bedingungen der Völkerkunde. In Mühlmann, *Methodik der Völkerkunde.* Stuttgart, Ferdinand Enke. Pp. 9–90.

— (1948), *Geschichte der Anthropologie.* Bonn, Universitäts-Verlag.

Müller, F. Max (1877), *Lectures on the science of language,* vol. 2., Ninth edition. London, Longmans, Green, and Co.

Needham, Joseph (1965), *Science and civilization in China,* Vol. 4, part 2. Cambridge, Cambridge University Press.

Nettelbladt, Sölve (1936), August Strindberg som svensk kulturhistoriker. In Sigurd Erixon and Sigurd Wallin (Eds.), *Svenska kulturbilder.* Ny följd. Tredje bandet. Del V och VI. Stockholm, Aktiebolaget Skonglunds Bokförlag.

Netter, Frank H. (1954), *Reproductive System. The Ciba Collection of Medical Illustrations,* Vol. 2. Summit, New Jersey, CIBA Pharmaceutical Products, Inc.

Neu, Peter (1972), Gesindetermine auf Eifelburgen im 16. und 17. Jahrhundert. In *Festschrift Matthias Zender.* Pp. 598–608.

The New Schaff-Herzog encyclopedia of religious knowledge (1909), *see* Sherman, Charles C.

Nordenskiöld, Erland Friherre (1919–1938), *Comparative ethnographical studies.* Göteborg, Elanders boktryckeri.

Nordland, Odd (1971), Traditional beer in Scandinavia and some reflections on taste, *Ethnologia Scandinavica,* 1: 166–171.

Norsander, Göran (1971), Heringssuppe. Zum ethnologischen Aspekt eines Gerichts, *Ethnologia Scandinavica,* 1: 171–184.

Noth, Martin (1938), *Das Buch Josua.* Tübingen, J. C. B. Mohr (Paul Siebeck).

Numelin, Ragnar (1947), *Fältforskare och kammarlärde: Drag ur socialantropologiens idéhistoria.* Stockholm, Hugo Gebers Förlag.

O'Danachair, Caoimhín (1970), The flail in Ireland. *Ethnologia Europaea,* 4: 50–55.

Paladi-Kovács, Attila (1971), Einige Bemerkungen über die Traggeräte der ungarischen Bauernschaft. In Szabadfalvi and Újváry, 1971. Pp. 409–426.

Palmer, Abram Smythe (1882), *Folk-etymology, a dictionary of verbal corruptions of words perverted in form or meaning or by false derivation* ... London, G. Bell and Sons.

Patai, Raphael (1947), Hebrew installation rites: A contribution to the study of ancient Near-Eastern-African culture contact, *Hebrew Union College Annual,* 20: 143–225.

— (1962), The ritual approach to Hebrew-African Culture contact, *Jewish Social Studies,* 24: 86–96.

Paul, Hermann (1960), *Prinzipien der Sprachgeschichte.* 6. unveränderte Auflage. Tübingen, Max Niemeyer Verlag.

Peake's commentary on the Bible, (1962), London, Thomas Nelson and Sons.

Peesch, Reinhard (1957), *Das Berliner Kinderspiel der Gegenwart.* Veröffentlichungen des Instituts für deutsche Volkskunde, XIV. Berlin, Akademie-Verlag.

— (1961a), Review of Atlas der deutschen Volkskunde, Neue Folge. *Deutsches Jahrbuch für Volkskunde* 7: 276–280. Berlin, Akademie-Verlag.

— (1961b), Review of Österreichischer Volkskundeatlas. *Ibid.:* 280–283.

— (1961c), Review of Atlas der schweizerischen Volkskunde. *Ibid.:* 283–284.

— (1961d), Review of Volkskunde Atlas voor Nederland en Vlaams-België. *Ibid.:* 284–286.

— (1961e), Review of Atlas över svensk folkkultur. *Ibid.:* 286–289.

— (1961d), Review of Polski Atlas Etnograficzny. *Ibid.:* 289–291.

Penniman, T. K. (1952), *A hundred years of anthropology.* Second edition revised. London, Gerald Duckworth & Co. Ltd.

Pessler, Wilhelm (1906), *Das altsächsische Bauernhaus in seiner geographischen Verbreitung.* Braunschweig, F. Vieweg & Sohn.

— (1909–1910), Deutsche Ethno-Geographie und ihre Ergebnisse, soweit sie kartographisch abgeschlossen sind. Ein Beitrag zur deutschen Ethnologie, *Deutsche Erde,* 1909, S. 194–201; 234–259; 1910, S. 3–9.

— (1910), System der Ethno-Geographie, *Mitteilungen der Anthropologischen Gesellschaft in Wien,* 60: 191–194.

— (1911), Ziele und Wege einer umfassenden deutschen Ethno-Geographie, *Wörter und Sachen,* 3: 56–65.
— (1912), Grundsätzliche Bemerkungen zu neueren ethno-geographischen Karten des Deutschtums. Ein Beitrag zur deutschen Ethnologie, *Deutsche Erde,* 1912: 34–40; 62–73.
— (1932), Die geographische Methode in der Volkskunde, *Anthropos,* 27: 707–742.
Petit Larousse illustré (1912), 79th ed., Paris, Librairie Larousse.
Philo (1855), *The works of Philo Judaeus* ... translated from the Greek by C. D. Yonge, vol. 3, London, Henry G. Bohn.
— (1906), Philonis Alexandrini opera quae supersunt, vol. 5. Leopold Cohn and Paul Wendland (Eds.). Berlin, Georg Reimer.
Plischke, Hans (1922), *Der Fischdrachen.* (Veröffentlichungen des Städtischen Museums für Völkerkunde zu Leipzig, Heft 6.) Leipzig, R. Voigtländer.
Plischke-Festschrift (1955), Lang, Werner, and Nippold, Walter, and Günther Spannaus, editors, *Von fremden Völkern und Kulturen, Beiträge zur Völkerkunde. Hans Plischke zum 65. Geburtstag,* Düsseldorf, Droste-Verlag.
Poirier, Jean (1968), Histoire de la pensée ethnologique. In: Jean Poirier, *L'ethnologie,* Encyclopédie de la Pléiade, Paris, Gallimard.
— (1969), *Histoire de l'ethnologie.* Paris, Presses universitaires de France.
Porteus, Stanley D. (1931), *The psychology of a primitive people: A study of the Australian aborigine.* London, Edward Arnold & Co.
Posen, Ida (1924), Gentilorganisation in der Bibel, *Sitzungsberichte der Anthropologischen Gesellschaft in Wien,* Jahrgang 1923–24: [3]–[6]. Vienna.
Preston, E. Noel (1970), Whither the foreskin? A consideration of routine neonatal circumcision, *Journal of the American Medical Association,* 213 (11): 1853–1858.
Raa, Eric ten (1970), Review of: *Die Masai* by M. Merker, *Anthropological Forum,* 2: 509–511.
Radcliffe-Brown, A. R. (1952), *Structure and function in primitive society: Essays and addresses.* Glencoe, Illionis, The Free Press.
Raglan, Lord (Baron Fitzroy Richard Somerset) (1949), *The origins of religion.* London, Watts & Co. (The Thinker's Library–No. 132.)
Rahmann, Rudolf (1957), Vier Pioniere der Völkerkunde. *Anthropos,* 52: 263–276.
The Random House dictionary of the English language (1969). Unabridged edition. New York, Random House.
Ränk, Gustav (1966), Från mjölk till ost: drag ur den äldre mjölkhushållningen i Sverige, *Nordiska museets handlingar,* 66.
Ranke-Festschrift (1968), *Volksüberlieferung, Festschrift für Kurt Ranke zur Vollendung des 60. Lebensjahres.* Herausgegeben von Fritz Harkort, Karel C. Peeters und Robert Wildhaber. Göttingen, Otto Schwartz & Co.

Rasmussen, Holger (1972), Der schreibende Teufel in Nordeuropa. In *Festschrift Matthias Zender*. Pp. 455–464.

— (1974), The use of seaweed in the Danish farming culture. In: *In Memoriam António Jorge Dias*, I: 385–398. Lisbon, Instituto de Alta Cultura, Junta de Investigações Científicas do Ultramar.

Ratzel, Friedrich (1885), *Völkerkunde*, vol. 1, Leipzig and Vienna, Bibliographisches Institut.

— (1890a), *Völkerkunde*, vol. 2. Leipzig and Vienna, Bibliographisches Institut.

— (1890b), *Völkerkunde*, vol. 3. Leipzig and Vienna, Bibliographisches Institut.

— (1891), *Anthropogeographie*. Part two, Stuttgart, J. Engelhorns Nachf.

— (1894), *Völkerkunde*, vol. 1, second edition. Leipzig and Vienna, Bibliographisches Institut.

— (1895), *Völkerkunde*, vol. 2, second edition. Leipzig and Vienna, Bibliographisches Institut.

— (1899), *Anthropogeographie*. Part one, second edition. Stuttgart, J. Engelhorns Nachf.

— (1904), Geschichte, Völkerkunde und historische Perspektive, *Historische Zeitschrift*, 93 (=N. F. 57): 1–46.

— (1909), *Anthropogeographie*. Part one, third edition. Stuttgart, J. Engelhorns Nachf.

— (1912), *Anthropogeographie*. Part two, second edition. Stuttgart, J. Engelhorns Nachf.

— (1921), *Anthropogeographie*. Part one, fourth edition. Stuttgart, J. Engelhorns Nachf.

— (1922), *Anthropogeographie*. Part two, third edition. Stuttgart, J. Engelhorns Nachf.

Realencyklopädie für protestantische Theologie und Kirche (1897). 3rd edition. vol. 2. Leipzig, J. C. Hinrichs'sche Buchhandlung.

Rhamm, K. (1908), Ethnographische Beiträge zur germanisch-slavischen Altertumskunde. Zweite Abteilung. Erster Teil: *Altgermanische Bauernhöfe im Übergang vom Saal zu Fletz und Stube*. Braunschweig.

Riemann, Erhard (1972), Fastnachtsbrauchtum in Ost- und Westpreussen. In *Festschrift Matthias Zender*. Pp. 195–226.

Riesenfeld, Alphonse (1950), *The megalithic culture of Melanesia*. Leiden, E. J. Brill.

Risch, Ernst (1973), *see* Bornemann.

Rikin, W. Mintardja (1973), *Ngabersihan als knoop in de tali paranti: Bijdrage tot het verstaan van de besnijdenis der Sundanezen*. Meppel, Racmo Offset.

Rivers, W. H. R. (1908), Totemism in Fiji, *Man*, 75 (=pp. 135–136).

— (1926), *Psychology and ethnology.* London, Kegan Paul, Trench, Trubner & Co.

Roberts, Helen (1936), Musical Areas in aboriginal North America. *Yale University Publications in Anthropology,* Number 12. Reprinted from the 1936 edition by Human Relations Area Files Press, New Haven.

Robinson, H. Wheeler, (Ed.) (no date), Deuteronomy and Joshua. *(The New-Century Bible).* New York, Henry Frowde.

Rohan-Csermak, G. de (1966), Review of Barabás 1963, *Folk-Liv,* 30: 71–73.

Róheim, Géza (1926), *Social anthropology: A psycho-analytic study in anthropology and a history of Australian totemism.* New York, Boni and Liveright.

— (1945), *The eternal ones of the dream: A psychoanalytic interpretation of Australian myth and ritual.* New York, International Universities Press.

Rohner, Ronald P. (1969), *The ethnography of Franz Boas.* Chicago, University of Chicago Press.

Roth, Walter E. (1897), *Ethnological studies among the North-West-Central Queensland aborigines.* Brisbane, E. Gregory, Government Printer.

Šach, František (1972), Übersicht über die Entwicklung der Bodenbearbeitung in Böhmen und Mähren. In Balassa, 1972a. Pp. 375–409.

Sandred, Karl Inge (1968), Notes on the distribution of some plough terms in modern English dialects, *Studia Neophilologica,* 40: 80–93.

Sarmela, Matti (1969), Reciprocity systems of the rural society in the Finnish-Karelian culture area with special reference to social intercourse of the youth. *FF Communications,* No. 207. Helsinki, Academia Scientiarum Fennica.

Schaff-Herzog (1909), *see* Sherman, Charles C.

Schiller, Friedrich (1789), Was heisst und zu welchem Ende studiert man Universalgeschichte? Inaugural Lecture, University of Jena.

— (1972), The nature and value of universal history: An inaugural lecture [1789]. *History and Theory,* 11: 321–334.

Schindler, Otto G. (1972), Nachrichten über Schwert-, Reif- und Steckentänze in Niederösterreich. In Beitl, 1972. Pp. 213–223.

Schmidt, Leopold (1957), Geschichtliche Grundlagen der Gerätekultur. *Carinthia* I, *Mitteilungen des Geschichtsvereins für Kärnten,* 147:773–802.

— (1972), *Leopold Schmidt Festschrift. See* Beitl, Klaus.

Schmidt, Wilhelm (1908), Totemism in Fiji. *Man* 8 (84) (=pp. 152–53).

— (1939), *The culture historical method of ethnology.* New York, Fortuny's.

Schurtz, Heinrich (1889), *Das Wurfmesser der Neger.* Leipzig.

— (1902), *Altersklassen und Männerbünde: Eine Darstellung der Grundformen der Gesellschaft.* Berlin, Georg Reimer.

Schwan, Chrétien Frédéric (1807), *Nouveau dictionnaire François-Allemand.* Tome II. Tübingen, J. G. Cotta.

Sebeok, Thomas A. (1966), *Portraits of linguists: A biographical source book for the history of western linguistics, 1746–1963.* Volume one: *From Sir William Jones to Karl Brugmann.* Bloomington and London, Indiana University Press.

Segal, Sheldon J. (1972), Contraceptive research: A male chauvinistic plot? *Family Planning Perspectives,* 4 (3): 21–25.

Seiler-Baldinger, Annemarie (1971), *Maschenstoffe in Süd- und Mittelamerika,* Beiträge zur Systematik und Geschichte primärer Textilverfahren. Basler Beiträge zur Ethnologie, vol. 9. Basel, Pharos-Verlag Hansrudolf Schwabe AG.

Sherman, Charles C. (1909), Circumcision. *The New Schaff-Herzog encyclopedia of religious knowledge,* vol. 3. New York and London, Funk & Wagnalls. Pp. 117–119.

Simoons, Frederick (1954), The non-milking area of africa, *Anthropos,* 49: 58–66.

— (1961), *Eat not this flesh: Food avoidances in the Old World.* Madison, University of Wisconsin Press.

Singer, Philip and DeSole, Daniel E. (1967), The Australian subincision Ceremony reconsidered: Vaginal envy or kangaroo bifid penis envy. *American Anthropologist,* 69: 355–358.

Slotkin, James S. (1965), *Readings in early anthropology.* Viking Fund Publications in Anthropology, No. 40. Chicago, Aldine Publishing Company.

Söderberg, Bertil (1956), *Les instruments de musique au Bas-Congo et dans les régions avoisinantes.* Statens etnografiska museum, Monograph series, Publication No. 3. Stockholm.

Söderström, J. (1938), Die rituellen Fingerverstümmelungen in der Südsee und in Australien, *Zeitschrift für Ethnologie,* 70: 24–47.

Speiser, Felix (1944), Über die Beschneidung in der Südsee. *Acta Tropica,* I:9–29.

Spencer, Baldwin and Gillen, F. J. (1899), *The native tribes of Central Australia.* London, Macmillan and Company, Ltd.

— (1904), *The northern tribes of Central Australia.* London, Macmillan and Company, Ltd.

— (1927), *The Arunta: A study of a stone age people.* London, Macmillan and Company, Ltd.

Spencer, Baldwin (1914), *Native tribes of the northern territory of Australia.* London, Macmillan and Company Ltd.

Spencer, Robert F. (1956), *An ethno-atlas.* Dubuque, Iowa, Wm. C. Brown Company.

Spencer, Robert F. and Johnson, Elden (1960), *Atlas for anthropology.* Dubuque, Iowa, Wm. C. Brown Company.

Steensberg, Axel (1973), *Oldtidslevn i New Guineas Hverdag.* Copenhagen, Forum.

Steiger, Arnald (1956), Letter of April 20. In Leser, 1968. P. 205.

Stern, Curt (1960), *Principles of human genetics.* Second Edition. San Francisco, W. H. Freeman.

Sternberg, Leo J. (1931), Die Ethnologie der Gegenwart: Neueste Fortschritte, wissenschaftliche Strömungen und Methoden. Ins Deutsche übertragen von A. Byhan, *Ethnologische Studien,* I: 215–258.

Stoll, Otto (1908), *Das Geschlechtsleben in der Völkerpsychologie.* Leipzig, Veit & Comp.

Strehlow, Carl (1913), *Das soziale Leben der Aranda- und Loritja-Stämme.* Part four, section one. Veröffentlichungen aus dem Städtischen Völker-Museum Frankfurt am Main. Frankfurt am Main, Joseph Baer & Co.

Strindberg, August (1882), *Svenska folk i helg ock soken, i krig och i fred, hemma och ute eller ett tusen år af svenska bildningens och sedernas historia.* Stockholm, C. E. Fritze's K. Hofbokhandel.

Struck, Bernhard (1909a), Zur Kenntnis afrikanischer Erdbebenvorstellungen, *Globus,* 95: 85–90.

— (1909b), Das Chamäleon in der afrikanischen Mythologie, *Globus,* 96: 174–177.

— (1922), Versuch einer Karte des Kopfindex im mittleren Afrika, *Zeitschrift für Ethnologie,* 54: 52–113.

Stuart, T. P. Anderson (1896), The 'Mika' or 'Kulpi' operation of the Australian aboriginals. *Royal Society of New South Wales, Journal and Proceedings* 30: 115–123 and Palte VI.

Stummer, F. (1954), Beschneidung. In Theodor Klauser (Ed.), *Reallexikon für Antike und Christentum,* vol. 2. Stuttgart, Hiersemann, pp. 159–169.

Sturtevant, Edgar (1947), *An introduction to linguistic science.* New Haven, Yale University Press.

Svensson, Sigfrid (1966), *Introduktion till folklivsforskningen.* Malmö, Bokförlaget Natur.

Szabadfalvi, József and Újváry, Zoltán Eds. (1971), *Studia Ethnographica et Folkloristica in honorem Béla Gunda. Műveltség és Hagyomány.* Studia Ethnologica Hungariae et Centralis ac Orientalis Europae, vol. 13–14. Debrecen, Kossuth Lajos Tudományegyetem.

Takács, Lajos (1972), Sensensicheln in Ungarn. In Balassa, 1972a. Pp. 561–582.

ten Raa, Eric (1970), *see* Raa, Eric ten

Terenteva, L. N. (1971), Erfahrungen bei der Erarbeitung regionaler historisch-ethnographischer Atlanten. In Szabadfalvi and Újváry, 1971. Pp. 69–85.

Theodoratus, Robert J. (1969), *Europe: A selected ethnographic bibliography.* New Haven, Human Relations Area Files Press.

Thurnwald, Richard (1921), *Die Gemeinde der Banaro.* Stuttgart, Strecker und Schröder.

— (1930), Papuanisches und melanesisches Gebiet, südlich des Äquators einschliesslich Neuguinea. Geordnet und bearbeitet sowie auf Grund eigener Forschungen ergänzt. In Erich Schultz-Ewerth and Leonhard Adam (Eds), *Das Eingeborenenrecht.* Stuttgart, Strecker und Schröder.
— (1940), Primitive Initiations- und Wiedergeburtsriten. In *Eranos-Jahrbuch* 1939. Zürich, Rhein-Verlag. Pp. 321–398.
Tiesler, Frank (1970), Tragbandschilde aus dem Hatzfeldhafen-Gebiet, dem Hinterland der Berlinhafen-Küste und dem Lumi-Gebiet, Nord-Neuguinea, *Jahrbuch des Museums für Völkerkunde zu Leipzig,* 27: 185–217. Berlin, Akademie-Verlag.
Tindale, Norman B. (1940), Results of the Harvard–Adelaide Universities Anthropological Expedition, 1938–1939: Distrikution of Australian Aboriginal Tribes: A Field Survey. *Transactions of the Royal Society of South Australia* 64: 140–231 and map 11.
Trager, George L. (1972), *Language and languages.* San Francisco, Chandler Publishing Company.
Turney-High, Harry Holbert (1949), *General anthropology.* New York, Thomas Y. Crowell Company.
Vakarelski, Christo (1972a), Traditionelle landwirtschaftliche Geräte der Bulgaren. In Balassa, 1972a. Pp. 339–374.
— (1972b), Bemerkungen zum Verhältnis von Sprach- und Kulturgrenzen auf Grund bulgarischen Materials. In *Festschrift Matthias Zender.* Pp. 99–105.
Valeri, Renée (1971), Study of traditional food supply in the south-west of France, *Ethnologia Scandinavica,* 1: 86–95.
Vařeka, Joseph (1972), Die Kulturgrenze des Fachwerkes in der Entwicklung des westböhmischen Bauernhauses. In *Festschrift Matthias Zender.* Pp. 237–248.
Vatter, Ernst (1925), *Der australische Totemismus. Mitteilungen aus dem Museum für Völkerkunde in Hamburg.* Vol. 10, Hamburg.
— (1926), Karten zur Verbreitung totemistischer Phänomene in Australien, *Anthropos,* 21 566–579.
Viires, Ants (1971), Über die Herkunft und Verbreitung des Krummholzes im Pferdegeschirr. In Szabadfalvi and Újváry, 1971. Pp. 427–449.
— (1972), Typen des Pferdeanspanns im Baltikum in ihrer räumlichen und zeitlichen Verbreitung. In *Festschrift Matthias Zender.* Pp. 249–263.
Vilkuna, Kustaa (1967), Zur Geschichte des finnischen Pferdes, *Studia Fennica,* 13.
— (1969), *Finnisches Brauchtum im Jahreslauf.* FF Communications No. 206. Helsinki, Academia Scientiarum Fennica.
— (1972), Die Heumahd mit der Hausense. In *Festschrift Matthias Zender.* Pp. 692–707.

Voget, Fred W. (1965), Forgotten Forerunners of anthropology. Paper Read at the 64th Annual Meeting of the American Anthropological Association.

— (1967), Progress, Science, History and Evolution in Eighteenth- and Nineteenth-Century Anthropology. *Journal of the History of the Behavioral Sciences* 3: 132–155.

— (1968), Anthropology in the Age of Enlightenment: Progress and Utopian Functionalism. Southwestern Journal of Anthropology 24: 321–345.

Vuorela, Toivo (1971), Die finnischen Würste, *Ethnologia Scandinavica,* 1: 126–130.

Waterman, John T. (1963), *Perspectives in linguistics.* Chicago, The University of Chicago Press.

Webster's third new international dictionary of the English language (1961). Unabridged. Springfield, Mass., G. & C. Merriam Company.

Weiss, Richard (1950), *Einführung in den Atlas der schweizerischen Volkskunde.* Erlenbach–Zürich.

— (1951), Sprachgrenzen und Konfessionsgrenzen als Kulturgrenzen. *Laos, Comparative Studies of Folklore and Regional Ethnology,* 1:96–110. Stockholm.

— (1952), Kulturgrenzen und ihre Bestimmung durch volkskundliche Karten, *Studium Generale,* 5: 363–373.

— (1962), Die Brüning–Napf–Reuss-Linie als Kulturgrenze, *Schweizerisches Archiv für Volkskunde,* 58: 200–231.

Weule, Karl (1899), *Der afrikanische Pfeil.* Leipzig, O. Schmidt.

Widenmann, (Stabsarzt Dr.) (1895), Beschneidung bei den Massai. *Zeitschrift für Ethnologie,* 27: (302)–(303).

Widstrand, Carl Gösta (1958), *African Axes. Studia Ethnographica Upsaliensia,* 15. Uppsala, Almqvist & Wiksells Boktryckery AB.

Wiegelmann, Günter (1970), „Materielle" und „geistige" Volkskultur. Zu den Gliederungsprinzipien der Volkskunde, *Ethnologia Europaea,* 4: 187–193.

Winthuis, J. (1928), *Das Zweigeschlechterwesen.* Leipzig, C. L. Hirschfeld.

Wolfram, Richard, Ed. (1959–1971), *Österreichischer Volkskundeatlas: Kommentar.* Wien–Köln–Graz, Hermann Böhlaus Nachf.

Yoder, Don (1965), The "Domestic Encyclopaedia" of 1803–1804. *Pennsylvania Folklife,* 14 (3): 10–27.

Zender, Matthias, Ed. (1958ff.), *Atlas der deutschen Volkskunde.* New series. Marburg, N. G. Elwert.

Zender, Matthias (1968), Kobold, Totengeist und Wilder Jäger. In *Ranke-Festschrift:* 415–427.

— (1971), Verbreitung von Sagenmotiven und Vorstellungen des Volksglaubens im Rheinland. In Cox, 1971a. Pp. 39–51.

— (1972a), *Zender-Festschrift.* See *Festschrift Matthias Zender.*

— (1972b), Mummereien im Rheinland. In Beitl, 1972. Pp. 261–270.

8 The Soviet Union

The continuity of the great Russian tradition

YU. V. BROMLEY

It is generally accepted that the term 'anthropology' in the English-speaking countries corresponds to the term 'ethnography' in the USSR and certain other countries of Europe.[1] In fact, the subjects of these disciplines are far from being identical, although they do have much in common and, in certain respects, converge. Indeed, their community of definition is sufficient to include Soviet ethnography in a book devoted to the anthropological *traditions*, in the broad meaning of the term, of various countries.[2]

In the Soviet Union, which is one of the largest multinational states, ethnographic studies have gained high momentum. The existence of the perennial traditions of ethnographic research in Russia before the 1917 Revolution was an important contributory factor to its current status. In the territory of the USSR, as elsewhere, information on the peoples and on the distinctive features of their lives can be found in the earliest records (e.g., in the writing of a seventh-century Armenian geographer, in the early twelfth-century Russian chronicle *Povest vremennykh let (The tales of time-honoured years)*, etc. Evidence of this kind has been building up for centuries.

The official recognition of ethnography as a science in Russia dates to the first half of the nineteenth century. The most important milestone in the establishment of the discipline was the founding of the Russian Geographical Society, including a department of ethnography in 1845

1. At present, the term ethnology is not used by Soviet scholars to designate either their specific studies of the peoples of the world or their theoretical work.

2. The term anthropology is usually employed in the USSR in a restricted meaning to designate physical anthropology only.

in Petersburg. Since that date, ethnographic materials have been gathered in a systematic manner; indeed, the Society maintained a special research program which distributed data throughout the country. Initially, the Society studied almost exclusively non-Russian nationalities of the Empire, but graudally it began to concentrate more on the Russian people as well.

One of the special features of ethnographic studies in the middle of the nineteenth century was the focus on folk art, including oral literature, which is traditionally called folklore in Russia. This brought ethnography into close contact with folkloristic studies; this connection has survived to this day and explains the influence of the mythological school on Russian ethnography.

Later in the century ethnography turned toward the natural sciences. A vivid illustration of this was the founding of the Society of the Lovers of the Natural Sciences in Moscow in the 1860s, with departments of ethnography and anthropology (anthropology was usually defined as the study of the variations of human physical type).[3] In 1867 the Society organized in Moscow the first All-Russia Ethnographic Exhibit and published a series of works dealing with the different peoples of Russia.

In the 1870—1880s ethnographers became more interested in the study of society, family and community, largely as a result of the abolition of serfdom in Russia. Russian populism *(narodnichestvo)*, which idealized the communal system, was coming into its own. The study of common law, among other things, attracted much attention, some of which was of great practical importance following the abolition of the landlords' jurisdiction. But the study of folk art was developing and had by this time become critical of the mythological school and the popular 'theory of borrowings' (V. V. Stasov, V. F. Miller). At the same time, scientific expeditions to non-European countries, especially N. N. Miklukho-Maklay's expedition to Oceania, were shaping Russian ethnography.

In the last decade of the nineteenth and in the early twentieth century, the ethnographic museums, both in the major cities (the Museum of Anthropology and Ethnography in Petersburg, the Dashkov Ethnographical Museum in Moscow and the Ethnographical Department of the Russian Museum in Petersburg) and in the provinces became more active. This, in turn, prompted interest in the study of the material life

3. The term was simultaneously used in a wider, philosophical sense.

of the people. Evolutionism, which had had something of an impact in the preceding decades, now became the leading ideological force in Russian ethnography (M. M. Kovalevsky, N. N. Kharuzin, L. Ya. Shternberg and others). Indeed, the Soviet ethnographic tradition, in significant part, originated in the prerevolutionary period. Already at that time the country's dominant nation (Russia) received no preferential treatment in the ethnographic literature relative to other nationalities, which was, of course, not the case in the colonialist countries. One reason for this was that the majority of the Russian people (peasantry) differed little from the peoples living on the fringes of the empire. It was utterly impossible for prerevolutionary progressive Russian ethnographers to divide peoples into 'historic peoples' and 'nonhistoric peoples'. Nor were the social conditions in Russia conducive to the spread of racist views in ethnographic science.

Yet until the Revolution of 1917, ethnography was in the hands of either independent amateurs or ethnographical societies with limited membership. No systems of state establishments had evolved, and at the few state-sponsored ethnographical museums research was on a small scale. Nor was ethnography taught at the universities.

Naturally, prerevolutionary ethnography had no single sociopolitical orientation. The central motive that kept the science alive was the contest between two principles, the conservative, and the progressive, that reflected the aspiration of the people. In ideological and theoretical matters, the discord among ethnographers was great. They could not even agree on the very definition of ethnography. Some held that the major task of ethnography was to discover laws of man's development at the lowest stages; others defined ethnography as a science dealing with the culture of primitive peoples in general and with the survivals of that culture; still others held that the task of ethnography was to study the peculiarities of the material and spiritual life of particular peoples.

The Revolution of 1917 radically changed the conditions in which ethnography was to develop in Russia. From the beginning Soviet ethnography had drawn upon the humanistic and democratic legacy of prerevolutionary Russian ethography, following the example set by its best representatives, who took an active part in building the new society. The Soviet ethnographers inherited a generous outlook and an authentic interest in all peoples of the world from their prerevolutionary fellow scientists.

The dominant force in the evolution of ethnographic studies in the post-revolutionary years was, however, the dovetailing of these studies with the practical requirements of the new social regime. The tasks set forth in Lenin's national policy and the need for radical changes in the society and culture of formerly backward peoples demanded thorough research into both the ethnic composition of the population and the peculiarities of the national cultures.

To tackle the problem, several new ethnographic centers (first in the form of various commissions and committees and later in the form of institutes as well) were set up in Leningrad and Moscow during the early Soviet years; in the interim most of the scientific societies in which the majority of ethnographic studies was carried on before the revolution resumed their work. Of immense importance in the organization of ethnographic research was the establishment, in 1926, of the journal *Ethnografiya* (*Sovetskaya etnografiya* after 1931). By the end of the 1930s, the network of ethnographic institutions was apparent, apart from the USSR Academy of Sciences' Institute of Ethnography,[4] there emerged corresponding research units in many of the Republics. Local scientific manpower, including the native nationalities, was hitting its stride. In the post-war period, this trend gained momentum; at present, ethnographic teams exist in all Soviet Republics and in several cities of the Russian Federation. The museums contribute greatly to such research.[5] Annual archaeological and ethnographical sessions of the Department of History of the USSR Academy of Sciences, at which the results of field research for the year past are summarized, are also of critical importance in the coordination of the activities of Soviet ethnographers.

The development of Soviet ethnography over the past half-century is not confined to the improvement of research and the training of scientific personnel. *The major trend has been the establishment and assertion of Marxist methods in ethnography*. This process in turn revolutionized our concept of ethnography prior to the Second World War and its status in the scientific world.

4. Named in 1947 after N. N. Miklukho-Maklay, well-known Russian explorer and ethnographer, the institute is at present the major center of ethnographic research in the USSR.

5. This applies to both the central museums (the Museum of Anthropology and Ethnography of the USSR Academy of Sciences and the State Museum of the Ethnography of the Peoples of the USSR) and the republican museums (those at Tbilisi, Tartu, and Lvov, for example). Numerous museums of regional ethnography also play an important role.

In the years directly following the 1917 Revolution, Soviet ethnography was noted for its widely divergent theories. During the late 1920s and early 1930s numerous discussions were held in order to clear up the theoretical disagreements and assert Marxist principles, both in ethnography and in most of the other humanities.

Despite extremism, the discussions were, on the whole, of value for the discipline. In the course of those debates, Soviet ethnography established itself as an historical science, with its own particular range of sources and specific problems.

One of the distinctive features of the ethnographers' activities in those years was the concentration on problems of social organization, especially on the various forms of patriarchal and patriarchal-feudal relations. This was in large measure a result of effort to master the Marxist–Leninist methodology.

Comparative-historical studies of the general problems of primitive organization, namely, the origin of exogamy, maternal clan and matriarchate and military democracy *inter alia* also attracted attention (the works of E. G. Kagarov, E. Yu. Krichevsky, A. M. Zolotarev, S. P. Tolstov and others). Data collection was undertaken on a wide scale, especially in the little-studied areas of the far north (B. G. Bogoraz-Tan, A. A. Popov, L. Ya. Shternberg, G. M. Vasilevich and others). At the same time, there was a tendency to narrow the concept of ethnography as a science, eliminating those of its fields that are formally beyond the framework of the historical sciences. As a result, studies of the contemporary way of life of the peoples of the Soviet Union and other countries came to a virtual standstill.

The immediate postwar years witnessed an increase in the range of ethnographic problems. In order to accommodate the practical difficulties of the final stage of the war, studies of ethnic borders and ethnic mapping began. In addition, work on the history of ethnography in the USSR and other countries was launched, and the study of contemporary ways of life was advanced as a primary problem. This, in turn, affected the overall definition of the subject matter of ethnography. More often than not, the boundaries of ethnography expanded too far, thus becoming ill-defined. Yet, a narrower definition of ethnography as a science dealing comprehensively with the peculiarities of the culture and society of peoples was also current.

The study of contemporary ethnic processes, both in the USSR and abroad, has become more prominent in recent years. Simultaneously,

the breadth of concrete sociological studies, including the study of ways of life, enabled ethnographers to concentrate on the ethnic specifics of present-day cultural and social processes. The concept of ethnography predominant in the USSR today is that of a science which deals with the stable and (ethnic) distinctive features of the every-day life of the peoples of the world; it includes ethnogenesis and ethnic history. Ethnography is regarded as one of the historical sciences, which, as we have said, was recognized during the prewar discussions. Such a classification makes the most of the consistent historicism of Soviet ethnography, derived from the methodology of historical materialism.

Although Soviet ethnographers deal primarily with contemporary peoples, they treat their ethnic peculiarities from an historical perspective – in the making, as it were. This inevitably brings into focus not only peoples living at the moment but also all peoples who have existed in the past. Ethnography thus embraces the whole of man's history, although exploring it from a certain viewpoint.

Regarding ethnography as one of the historical sciences, Soviet ethnographers at the same time consistently emphasize its peculiar position with regard to the sciences at large. This peculiarity is, in the first place, due to the fact that in order to identify distinctive and typical traits in the life of each people, we must study this life as a whole, not only the sociocultural sphere in which these traits make themselves manifest most vividly. Hence, the ethnographic study of peoples overlaps with many of the 'departmental' social and natural sciences. Indeed, no sharp line of demarcation can be drawn between certain of these disciplines and ethnography. It is not accidental that in this country, as in many others, a whole array of borderline disciplines has come into existence: Ethnic anthropology, ethnic geography, ethnic linguistics, etc. Further, Soviet ethnography remains closely connected with the study of folklore. Special emphasis on the study of contemporary forms of social life makes ethnography, in the opinion of Soviet scientists, cognate with concrete sociological studies. Indeed, in recent years a new science—ethnosociology—has been emerging at the juncture of the two sciences. The main object of the new science, Soviet schloars believe, is to study the interactions of ethnic and social processes with regard to the specifics of ethnic process in various social groups, on the one hand, and the specifics of social changes in various ethnic environments, on the other.

In our opinion, the science of ethnography is also remarkable for its

unique methods, namely, its integrated use (apart from materials of all the historical sciences, including archaelogy) of data derived from diverse branches of knowledge; its broad application of the method of direct observation of the present-day life of peoples, with special field research undertaken to this end. The dozens of expeditions commissioned annually by the central and local ethnographic institutions in all regions of the. Soviet Union have harnessed a wealth of data, most of which have been published and exhibited.

During the last half-century, then, the major research trends in Soviet ethnography have become evident. One of these trends may be defined arbitrarily as the historico-ethnographic study of the peoples of the world The problems here range from ethnogenesis to contemporary socio-cultural and national processes; thus, Soviet ethnography converges with what is known in the English-speaking countries as cultural anthropology.

The problems of ethnic history, including ethnogenesis, occupy a special place among the historico-ethnographical studies of peoples; investigation of these problems is of immense significance in the Soviet Union. For it has become clear that all present-day peoples have grown from differing ethnic components and have a mixed composition, thereby refuting the racist and chauvinist allegations of 'racial purity', 'primeval ancestors' and 'national exceptionality'.

For many decades Soviet ethnographers have been studying the problems of ethnogenesis in collaboration with anthropologists, archaeologists and linguists. In the past, ethnogenetic problems were settled mainly on the strength of linguistic evidence and were largely confined to the history of languages—their origin and distribution, with priority going to the theory of the migrations of peoples. Later, certain primitive-migratory theories of the origin of peoples were revised under the impact of N. Ya. Marr's linguistic theory. The tables were turned, and the role of migrations was denied altogether. The well-known discussion of the problems of linguistics in 1950 helped to clear up this misunderstanding. Further, the wider use of anthropological evidence enabled us to call into question the traditional concept that the majority of migrations had entailed nearly complete destruction or ousting of local aboriginal populations. Thus, ethnogenetic studies by no means evolved in a straightforward manner. Yet the integrated approach, which gained wide appeal in the postwar years, has proved fruitful in the investigation of the origin of separate peoples. What we know today about these origins corresponds more to historical reality than to the views held two or three decades

ago. This is equally true of peoples inhabiting the most diverse historico-ethnographic regions of our country: The Baltic area, the Volga area, the Caucasus, Central Asia, Siberia, the Far East and elsewhere. Soviet scholars, I should note, also concentrated on the problem of the origin of foreign peoples, especially those of America, Asia, Australia and Oceania.

The study of the history of the ethnic and, above all, sociocultural pecularities of individual peoples monopolizes the activities of Soviet ethnographers. In the course of study, they often have to scan the culture and society of a given people as a whole, for one cannot see the general without discovering the specific. Furthermore, the ethnographer usually finds himself at a disadvantage when studying peoples who have no written language, and he therefore has to document all aspects of the life of such peoples.

In their study of the cultures of ethnic communities, irrespective of the size of their populations, Soviet ethnographers have gone a great way toward revealing what contributions different peoples of the world have made to the overall cultural treasury of mankind. Especially indicative in this respect is the study of material culture.

During the past 50 years many special studies have been published on the history of farming technology, settlements, dwellings, clothes and food, embracing many, if not all, peoples of the USSR, as well as several foreign nations. Historico-ethnographical regional atlases are prepared by ethnographers working jointly at the centers and in the Republics in order to summarize the whole body of data accumulated on the history of the material culture of the peoples of the Soviet Union. In 1961 an atlas devoted to the peoples of Siberia appeared, and in 1967 an atlas entitled *The Russians* was published. In this latter publication, the major components of the material culture from the middle of the nineteenth to the early twentieth century of the Soviet Union's largest people (the Russians) are characterized by a cartographic method. Each of the phenomena in the atlas is charted dynamically (in the pre-Reform period and prior to the 1917 Revolution). Thus, the atlas gives a clear idea of the major features of the evolution of the Russians' material culture in the prerevolutionary period. The charts (75 of them) are accompanied by numerous tables and short explanatory notes. Atlases devoted to other peoples of the USSR are now in preparation.

As for spiritual culture, the attention of Soviet ethnographers is above all drawn to mass folk art, a field which they explore together with folk-

lorists and art students. Ethnographic studies of the different forms of the profoundly individual folk arts of the minority peoples have made special progress.

Religion is known to mould many of the features of the contemporary life of one people or another. Largely for this reason, Soviet ethnographers consistently concentrate on the various aspects of religion. They have contributed most of all to the study of the early faiths and cults noted for their diversity of form, as well as to the problems of the origin and classification of religions. In recent years the study of such traditional ethnographical subjects as folk morals and customs has become quite lively.

The deciphering of certain long-forgotten written languages is an important part of the ethnographic study of culture. The most important achievement in the field of ethnolinguistics is, of course, the work on the deciphering of the ancient Maya script. Research is also under way to study the script of Easter Island, the proto-Indian texts originated by the forefathers of the Harappian culture, and the scripts of the Kitans and Jurjans.

There are many monographs and special collections reflecting the results of the historico-ethnographic studies of both individual peoples and whole regions of the Soviet Union. Of particular importance, among works of this kind, are the historico-ethnographic monographs concerned with minor peoples who possess no written language. For it is the ethnographer, commanding the materials of direct field observation, who can best reconstruct the history of such peoples by drawing upon all sources, including archaeological data. Indeed, the ethnographers' dedication to this task has helped to restore the history of a few dozen peoples of our country who had no written language at all or had it in a rudimentary form in the past. In the last five years alone, historico-ethnographic monographs on the Ulchi, Veps, Archintsy, Nivkhi, Chukchi, Nentsy, Orochi and others have appeared. There are also several historico-ethnographical monographs devoted to certain foreign peoples, in particular the peoples of Latin Amesica.

Today, the study of contemporary ethnic processes is one of the central subjects of Soviet ethnographic research. The reason is obvious enough. The sweeping economic, social and political changes that have taken place in the post-war period all over the world, and in particular in Africa, Asia and Latin America, catalyzed national and ethnic processes. As a result, interest in the ethnic life of the peoples of the world has deve-

loped to an unprecedented degree. Soviet ethnographers could not, of course, remain indifferent.

They concentrated on the study of the sociocultural process in its ethnic individuality. Work in this field was begun as early as the 1920s, dictated by the practical need for reconstructing and collectivizing the backward fringes of the country. By the end of the 1930s, research in this field, as we have noted, was nearly at a standstill, and the ethnographers' efforts shifted to the field of archaic studies. Revived in the late 1940s, the ethnographic study of the contemporary cultures of the peoples of the Soviet Union was primarily descriptive. Further progress was made in this area in the late 1950s and early 1960s. Several monographs on the collective-farming cultures of the Russians, Tadjiks, Uzbeks, Ukrainians, Latvians, Kirghiz and other peoples of the Soviet Union were prepared and published in this period.

The scope of research gradually broadened to include the life of workers; later, the entire urban population became a prominent subject of inquiry. Methods of investigation improved as well. The study of the complex processes of modern life demanded the gathering of data on a massive scale; therefore, scientific questionnaires and computer techniques were employed. The question of what kind of ethnographic inquiry was to be undertaken among contemporary, highly developed societies has also received attention in recent years. At the same time, the need for finding a relationship between ethnography and sociology in the study of contemporary sociocultural processes is becoming increasingly clear. Accordingly, tentative yet vigorous steps are being taken in the area of ethnosociological research on the peoples of the USSR.

Of equivalent scientific interest are the ethnographic studies (drawing upon the evidence of linguistics and census records) of national consolidation, including the breakdown of isolation and seclusion, the confluence of separate cognate ethnic formations into nations and the merging of small national groups in close-knit ethnic communities. Studies of such vital ethnic processes as nationally mixed marriages, relationships between the native language and ethnic self-consciousness, bilingualism, etc., have also progressed in recent years.

The study of modern sociocultural and ethnic processes abroad, particularly in Asia and Africa, are gaining momentum. Work on contemporary ethnic dynamics in the USA, Canada and Latin American countries has begun, along with similar studies of Western European peoples.

Ethnodemographic and ethnogeographic studies of modern peoples have become popular in recent years in the USSR, and several methods of charting various ethnic and demographical indexes have been devised. The new techniques permit ethnic charts of all parts of the globe to be completed within a relatively short time; little-known regions have received special attention. Also, the synoptic chart – 'The peoples of the world' – and *The atlas of the peoples of the world* which incorporates the results of many years' of research, have been published. The most significant outcome of ethnodemographic research is the summarizing work entitled *The numbers and distribution of the peoples of the world,* which provides a detailed description of the national composition of the population of all countries of the world, as well as the numbers of the separate peoples and their distribution.

The investigation of the concrete ethnic and sociocultural dynamics of different peoples of the world involves, in the Soviet Union, the elaboration of certain general matters. Thus, in order to understand the laws of development of specific features of culture among separate peoples, Soviet ethnographers developed a theory of economic-cultural types and historico-ethnographical regions. In the first case, the reference is to distinct complexes of economy and culture, each of which is simultaneously basic to several peoples sometimes initiating different areas of the *ecumene.* Although these complexes build up independently among different peoples, they prove to be uniform in type by virtue of being on approximately the same socioeconomic level of development in similar environments. By historico-ethnographic (or historico-cultural) regions are meant separated areas of the *ecumene*, the populations of which reveal similar sociocultural peculiarities by virtue of the community of socioeconomic development, sustained intercourse and reciprocal influence.

In recent years Soviet ethnographers have also concentrated on the theoretical elaboration of those ethnic processes and problems essential for an understanding of such general laws as the interrelation of cultures, and the role of continuity and innovation in the elaboration of culture. Soviet ethnographers also took an active part in the theoretical work that has recently been launched on the typology of ethnic communities. Special emphasis in this field is placed on the notion of *ethnic community itself.* Soviet ethnographers understand it as having a wider referent than the term *people* because ethnic community can be applied both to a group of peoples cognate in language and culture and to that segment of a people which has a distinct language and culture.

Apart from the historico-ethnic studies of the peoples of the world, the study of the history of society is a field of primary importance in Soviet ethnography. This corresponds roughly to the science known as social anthropology in the English-speaking countries, but it is, of course, evolutionary rather than functional in perspective.

During the early, archaic stages of social development, economic production and family life are welded together, and ethnic consciousness, as we know, permeates all life. For this reason, the ethnographic study of pre-class and early class societies usually covers all areas of culture. This, in turn, makes it possible to use ethnographic evidence obtained in the course of direct observation of archaic phenomena in order to reconstruct primitive history. Indeed, the ethnographic study of military democracy and domestic serfdom has been of major importance in revealing the actual means of transition from pre-class to class society.

The identification of specimens of man's pre-class stages plays a major role in the elaboration and substantiation of the Marxist conception of social development. From the very beginning, Soviet ethnographers have focused their attention on primitive history, a field they explored in collaboration with archaeologists and anthropologists. Over the past decade, a vast amount of new evidence indicating the historical versatility of clan organization has been collected and put into scientific circulation by Soviet ethnographers; for example, the wide occurrence of dual organization as one of the basic features of the primeval clan has been proved. The later forms of primitive communal organization have been investigated more thoroughly: the complex structure of the patriarchal clan has been ascertained, the typology of the extended family undertaken and evidence on the segmental form of this family—the so-called patronomy—has been classified and summarized.

As the materialistic conception of primitive history developed and crystallized, it became clear that some of the particular contentions advanced by L. H. Morgan were no longer valid. The elaboration of these contentions, which began as early as the prewar years and has gained even more popularity in recent times, is important because the attempts to disprove the materialistic conception of primitive history is usually connected with the ascribing of Morgan's erroneous notions to Marxism. In light of present-day ethnographic evidence, the pattern of development of primitive family-marriage relationships has been clarified, disproving Morgan's hypothetically reconstructed states of consanguine family and punaluan family. It is also worthwhile to mention the refusal of many

Soviet ethnographers to accept Morgan's view that the origin of exogamy is due to conscious efforts to avoid the harmful consequences of incest. However, as yet there is no single viewpoint among Soviet ethnographers concerning exogamy or the origins for incest taboos, both of which are problems of vital importance in understanding the causes and mechanism of the transformation of the human herd into the clan community.

Recent ethnographical and archaeological findings have also made it necessary to specify further, as F. Engels foresaw, Morgan's periodization of primitive history. In the course of the postwar discussions, a number of new periodizations have been suggested. Although the question has not yet been settled, the major result is already obvious. It is accepted that this periodization must first, be based on sociological rather than cultural-historical indexes. Among other matters that have given rise to lively discourse are the relationships between clan and clan community, the sequence of the rise of matrilineal and patrilineal descent, the historical context of nonlocal marriage, the pattern of early forms of marriage relations and so on.

The study of archaic forms of social life as they have survived in class and, above all, early-class societies is closely associated with the elaboration of the problems of primitive organization. Perhaps the most important contribution that has been made by Soviet ethnographers in this field is the study of nomadic social organization. It is also necessary to mention the ethnographers' active participation in the discussions on early-class society, as well as their contribution to the study of various historical types of community. The introduction of new data on the various types of community, as these function in diverse regions of the globe, is another subject of attention.

It is important to emphasize the inquiry, which has become especially lively in recent years in the USSR, into the nature of ethnography itself. Foreign studies of an ethnographic nature have also been widely circulated in the postwar decades, including works in social and cultural anthropology, ethnology and so on. Several papers have been published in periodicals, and special collections, e.g., *Modern American Ethnography*, are devoted to their analysis.

It is also necessary to draw attention to the struggle of Soviet ethnographers against racists, who occasionally try to derive their arguments from not only anthropology, but also ethnography. Although of long standing, the activity of Soviet ehtnographers in this field recently

took a new lease on life when the collective works *Against Racism* (1967), *Racism. NO!* (1968), as well as the collection entitled *Documents Exposing Racism* (1968), were published.

Soviet ethnographers' international contacts have widened in the postwar years. This has been due largely to the Seventh International Congress of Anthropological and Ethnographical Sciences, held in Moscow in 1964 and attended by about 2,000 Soviet and about 1,000 foreign scholars from 55 countries. The Soviet delegation took an active part in the work of the Eighth International Congress of Archaeological and Ethnographical Sciences, held in Tokyo-Kyoto in 1968, and presented more than 40 reports on a wide variety of ethnographic, anthropological and folkloristic matters.

The cognative significance of historico-ethnographic knowledge predetermined the Soviet ethnographers' immediate concern with the descriptions of peoples in the form of summarizing works, textbooks, and popular science fiction. In particular, the formulation of textbooks and teaching aids has made appreciable progress in recent years. *Essays in General Ethnography*, a five-volume, popular science publication, covering all the peoples of the world, has been completed.

The thirteen volume series *The Peoples of the World*, under the general editorship of S. P. Tolstov, was completed in 1966 and, in a sense, sums up the development of Soviet ethnography. Each volume in this series furnishes detailed information based on the most recent sources about the ethnic composition of the given population, the sociocultural peculiarities of separate peoples, as well as historical perspectives on their cultures from ancient times to the present day. In their efforts to keep in focus man's multifaceted image, Soviet ethnographers have contributed to better international understanding.

9 Italy
Cultural anthropology reconsidered

VINIGI L. GROTTANELLI

If a certain degree of discontinuity in time is not to be considered an impediment to the recognition of a national tradition, Italian anthropology can well be traced back to the first century B.C. C. Julius Caesar's (102–44 B.C.) commentaries on the Celts are an early example of a direct field report on preliterate tribes; book V of T. Lucretius Carus' (ca. 94–55 B.C.) *De rerum natura* contains *in nuce* the theory of evolution; and P. Cornelius Tacitus' *Germania,* written in A.D. 98, is surely the finest tribal monograph prior to the nineteenth century.

In mediaeval Europe, Italians were the first to follow the example of Arab scholars in giving stirring, sometimes accurate, first-hand accounts of exotic countries and cultures. Fra Giovanni da Pian de 'Carpini's *Historia Mongalorum* (1245–1247), followed a generation later by Marco Polo's *Il Milione* (1298), opens a new phase in Western ethnographical literature. From this period to the days of the Renaissance navigators and discoverers – Columbus, Vespucci, G. and S. Caboto, Verrazzano, A. Pigafetta, etc. – and down to the times of the great missionary reports, of which Matteo Ricci's (1552–1610) and G. A. Cavazzi's (1621–1680) are the most outstanding instances before Cardinal Massaia, there appears to be no solution of continuity in the interest taken by Italian elites in what we would now term anthropological matters – the variation in custom, law and culture among peoples of distant countries and the corresponding racial differences. G. B. Ramusio's *Delle navigationi et viaggi* (1550–1559) is the early model of a type of exotic literature that remained popular in Europe for three centuries, just as in more recent times the 21 bulky volumes of G. Ferrario's *Il costume antico e moderno* (Milano, 1817–1834) are probably the largest collection of ethnographical

information assembled up to his days, and indeed for several decades to come.

All this does not, of course, add up to saying that anything like an 'anthropological tradition' in the scientific sense of the term existed in Italy, any more than it did in France or elsewhere in Europe, before the second half of the nineteenth century. The stage was set, as it were, for such a tradition to materialize and to flourish; that it did so late, and so incompletely, is all the more regrettable because hardly anywhere else between the sixteenth and the eighteenth centuries did the cultural and intellectual ground seem more fertile.

Most of the preanthropological literature of this period was descriptive. But through it there runs the thread of cultural comparison and evaluation, with persistent attempts – sometimes naive or absurd, but occasionally far-sighted – at explaining the causes of variation or similarity in race and culture. This is particularly evident, for instance, in the admirable *Lettere* written from India in 1583–88 by the learned Florentine merchant Filippo Sassetti or in Francesco Carletti's (1573–1636) *Ragionamenti* on his journey round the world, the first successful attempt by a private traveler, and even more so in Matteo Ricci's work on China.

Some contemporary authors have emphasized the importance of G. B. Vico (1668–1744) in the rise of anthropological theory. It probably lies not so much in his so-called natural determinism or in his search for cultural regularities as it does in two of the basic principles already stated in the first (1725) edition of his *Principi d'una scienza nuova d'intorno alla natura delle nazioni (New science on the nature of nations)*. This first principle is that scientific research cannot attain the bedrock of a truly 'savage' state of mankind, all known nations or tribes displaying some common features of civilization, such as faith in a divine providence, family based on marriage; and belief in afterlife proved by the presence of burial rites. The second principle is that the development of culture does not proceed along a continuously progressive line; rather, it shows decline as well as advancement, 'courses' and 'recourses'. As Vico himself declares, his work was prompted by his dissatisfaction with the trends in the approach to the study of mankind prevailing in his days; they were either merely historical or merely philosophical – either 'idiographic' or 'nomothetic', as he might have worded it had such a terminology been possible in his days. In more ways than one, Vico's sociocultural evaluation of human development 'may be said to have

by-passed the eighteenth-century' and to have exerted its influence only in the following, as Bidney (1953, p. 307) has pointed out; one could add that philosophers have been aware of this influence more than have anthropologists.

Later Italian writers of the eighteenth century show more conservative views, as for example the Venetian lawyer Giuseppe Antonio Costantini (1747) who first attempted a worldwide comparison of the Great Flood myths in order to prove the historical truth of the Deluge, or F. S. Gilij from Spoleto, a missionary in the Orinoco basin, in his critical work on American Indians (1780).

By far the most remarkable book published in Italy in the late decades of the century, and the first one in which both the spirit and the subject matter are consistently anthropological, is F. A. Grimaldi's *Reflections on the inequality of men* (1779–1780). This curious work, strangely ignored by anthropologists in Italy and abroad, aimed at refuting the utopian theories of the day on 'natural man', Rousseau being the main target of its sound, often scathing criticism. But it also anticipates by decades concepts and trends that were to become fashionable in the nineteenth century, such as Bastian's theory of *Elementar-* and *Völkergedanken,* the emphasis on systematic comparison of cultural data (e.g., skull deformation, cannibalism, the couvade, etc.) and a positivistic approach to their study.

It has been debated whether the term 'anthropology' was first coined by an Italian (G. Capello, 1533, *L'antropologia ovvero ragionamento della natura umana,* Venice) or by a German called Hundt, who gave his treatise on anatomy (Leipzig, 1501) the somewhat different title *Anthropologeion.* But the controversy is irrelevant, as neither author wrote anything approaching what we now mean by anthropology (Penniman, 1953). Much later, another Venetian was among the first to use the term 'ethnography' in the title of a book (A. Balbi, 1826, *Atlas ethnographique du globe,* Paris); but, again, the name is misleading because Balbi, a geographer, was speaking mainly of linguistic families.

Learned societies and their programs

Indeed, in the crucial central decades of the nineteenth century, when anthropological studies were gradually emerging from the sphere of literary curiosity to the dignity of the fully scientific discipline, Italy's

intellectual life was seriously handicapped. The energies of her educated classes were absorbed in the task of ensuring the country's political independence and unity, and once these goals were attained (1861, proclamation of the kingdom of Italy; 1870, occupation of Rome), they were concerned with the even more arduous task of providing the young state with uniform laws and structures after centuries of division.

The first Italian chair of anthropology *('antropologia')* was established in 1869–1870 in Florence, then the temporary capital of the kingdom. According to the already accepted terminology, which has remained fundamentally unchanged to this day, the title implied the teaching of physical anthropology. The chair was entrusted to P. Mantegazza, physician and pathologist, and ardent follower of Darwin. In 1870 the Società Italiana di Antropologia e di Etnologia was founded in Florence. Its program, condensed in the form of a long letter dated 15 January 1871, was printed and circulated as a folder to scholars. A literal translation of its main points follows.

> The undersigned, having founded in Florence a Society of Anthropology and Ethnology, invite all those who in Italy are devoted to these studies to participate in it, so that uniting our dispersed forces we too may contribute to the development of those sciences which nowadays call for the attention of all thinkers in Europe. They [the undersigned] intend to cultivate these studies with the utmost broadness of views and, while they will concern themselves with the general problems of science, they intend to devote the best part of their efforts to the study of the ancient and modern peoples of Italy. As scholars and as Italians the undersigned consider it their almost supreme duty to promote an association intended to bring light to Italian ethnology, so rich and unfortunately so little known. This aim they hope to achieve by fostering also in Italy, in this class of studies, the experimental method that has proved so useful in all modern sciences ... including ethnology and anthropology.

The letter is signed by 24 persons, including Cesare Lombroso, the founder of criminal anthropology, Pellegrino Strobel, the discoverer of North Italian *terremare*, Bartolomeo Gastaldi, the eminent geologist and palaeontologist, Luigi Pigorini, the future founder of the Rome museum, and four members of parliament, one of whom was Paolo Mantegazza himself. Several of these scholars had in fact contributed to the first volume (1871) of the Society's journal, *Archivio per l'Antropologia e la Etnologia*, which included, along with articles on physical anthropology and social psychology, a study on Etruscan skulls, one on prehistoric implements from Apulia and Calabria, another on Buddh-

ism, as well as an extensive paper by E. H. Giglioli on the Tasmanians and one by F. Finzi on 'Myth and religion in ethnology'.

The interdisciplinary character of the Society and of its journal was obvious at the offset, but a schism soon developed. The International Congress of Anthropology and Prehistoric Archaeology held in Bologna in 1870 had brought new self-assurance to prehistorians and had encouraged them to assert their independence as representatives of a separate science in its own right. In 1875 P. Strobel, G. Chierici and L. Pigorini launched in Rome the *Bullettino di Paletnologia Italiana;* a chair of Palaeoethnology was created in 1876 by the University of Rome for Pigorini, who was to teach there for a period of almost 40 years. In the same year, Pigorini founded and opened the Museo Preistorico-Etnografico (later to become the Royal and then National Museum) in the same palace of the Collegio Romano that from the seventeenth century onward had housed the Museo Kircheriano – possibly the oldest institution of its kind in the world.

The idea that led to the foundation of the new double museum–similar but soon superior to the one already established by Mantegazza in Florence's Palazzo Nonfinito–was an evolutionary one. As Pigorini himself later explained in a paper of 1891, it would not be possible to understand the arms and implements of prehistoric men and to discover their ways of life except by comparing them with contemporary primitive peoples. One must, he said, 'proceed from the known to the unknown, make one's way from what may be seen to that which is lost in the depths of the past, know the life of modern savages in order to comprehend that of prehistoric man' (quoted in Barocelli, 1937, pp. 3–4). It is quite obvious that he did not acquire ethnographic collections for their own sake but rather as items useful for comparison; this holds true for the early stages of Pigorini's activity, but his correspondence, preserved in the Museum's archives, shows that as the years passed he became a relentless acquirer of ethnographic objects, even those having no possible relationship with prehistoric weapons and tools.

The next event of relevance to our subject was the founding in 1893 of the Societa Romana di Antropologia. Once more, the birthplace was the Collegio Romano, but the statute of the new society makes it clear that prehistory is not ostensibly included among its main interests: 'The scope of the Society is the study of physical anthropology, ethnology, experimental psychology, and sociology' (*Atti della Società Romana di Antropologia*, 1893, p. 3). The main promoter was Giuseppe Sergi, a

Sicilian physician whose early interests had been in the field of psychology and, who was later to become the head of the Roman school of physical anthropology for many years (1894–1916). The Society was later transferred to the University of Rome, and in 1937, long after Sergio Sergi had succeeded his father to the Roman Chair of (physical) Anthropology, it changed its name to Istituto Italiano di Antropologia.

Here again, the Society's practical orientation is shown by the type of papers accepted for publication in its journal *(Atti della Società Romana di Antropologia*, vols. I–XV, then *Rivista di Antropologia)*. The first volume (1893) includes among its major contributions a paper by E. Brizio on Italic burial sites and one by L. Livi on anthropometry in the Italian army but no articles on ethnological subjects; the second volume presents papers by S. De Sanctis on the phenomena of contrast in psychology and by G. Sergi on Pygmies in Europe, side by side with an ethnographic description of the Chamacoco by G. Boggiani, an article by G. Ciraolo Hamnet on aspects of murder in Calabria and a curious essay by P. Mingazzini on collecting among animals *(Atti della Società Romana di Antropologia*, 1893, *fassim)*. It should be noted that under the guidance of S. sergi (long retired from active teaching, but still a secretary general and editor of the journal) and in the face of difficulties due to the ever-increasing tendency toward technical specialization in the various sciences of man, the Istituto Italiano di Antropologia has tried to remain faithful to the holistic principles of its founders.[1]

Pioneers of field work

But quite apart from the activities of learned societies, the period from 1870 to World War I was notable in Italy as elsewhere for a series of direct contributions to ethnographic knowledge, thanks to the voyages of pioneers who may well still be called 'explorers' in the old meaning of the word. I shall restrict myself to quoting a few names. In the wake of C. Piaggia, who had preceded Schweinfurth in the discovery of Azande and Mangbetu countries on the Sudan-Congo borders (1863–1865), G. Miani concluded his tragic expedition (1872) by sending to Italy the first two African Pygmies ever brought to Europe; they were studied

1. For S. Sergi's conception of anthropological sciences, *cf.*, 1944–1947, especially pp. 77–80.

by Italian anthropologists after Miani's death. R. Gessi visited the peoples of the Lake Albert and Bahr el-Ghazal basins (1876–1879), and G. Casati explored the Nile-Welle watershed and the areas south of Lake Albert (1880–1890). O. Beccari traveled extensively in little-known regions of New Guinea, the Aru and Kei islands, Celebes and the Moluccas (1871–1875), and his former companion, L. M. d'Albertis, was the first to visit the Fly River Papuans (1876–1877). G. B. Cerruti spent half his lifetime in the interior of Sumatra, Malaya, and other parts of S. E. Asia (from 1881 onward) studying, *inter alia*, the lalnguages and customs of the Sakai and Semang. We owe to E. Modigiani excellent first-hand studies of the Batak and of the Nias and Engano islanders (1886–90) and to G. Boggiani equally precious reports and ethnographic collections from the Mato Grosso and the Great Chaco, especially from the Caduveos (1888–1901).

Men such as these, like their contemporaries from other nationsl contributed significantly to the foundations of factual anthropologica, knowledge of their day and, indeed, of ours. Yet none of them had- or possibly could have, received any real grounding in matters anthroe pological – a nonavailable luxury in their youth. Gessi, like V. Bottego – thi explorer of Jubaland and southern Ethiopia – was an army officer; Beccar, was a botanist; d'Albertis was an amateur ornithologist and a gentleman of leisure; A. Cecchi was a captain in the merchant service; Miani had been trained as a musician; and Boggiani was essentially an artist. Only Modigliani had begun his scientific career as a prehistorian. The ambition to explore, or mere love of adventure and danger, had prompted their journeys. Some of them brought back conspicuous ethnographic collections and wrote extensive reports on the people they had met or lived with; some did neither, having met with premature, often violent, death in the field.

The value of their contributions obviously varies according to their personal peculiarities; yet this heterogeneous group of self-taught, militant ethnographers reflects the mentality of their generation, and their work is therefore meaningful for our present purpose. Their recording of cultural data is generally idiosyncratic; it hardly ever occurs to them that exotic societies may be studied as functioning organisms in a dynamic context. On the other hand, they are often keen in noticing cultural parallels, suggesting ethnic affinities and classifications and tracing racial and linguistic frontiers. With few exceptions, their outlook and interests are mainly geographical and naturalistic.

ITALIAN ETHNOGRAPHY AND FOLKLORE

One of this group of pioneers, Lamberto Loria (1855–1913), deserves special mention. Having graduated in mathematics at the University of Pisa, he traveled extensively in three continents, studying on different occasions the Teke of Turkestan, the highland tribes of Eritrea and the Massim of S. E. New Guinea. He visited, among others, the Trobriand islanders[2] some 30 years before Malinowski went there. At the age of 60, he experienced a sudden change of interest, which was not without effect on the development of anthropological studies in Italy. To quote his own words (Loria, 1912a, p. 9):

> In 1905, before sailing for Africa for my studies, I had to go to Circello del Sannio [a small town of some 3,000 people in the province of Benevento]. And there I got the idea of abandoning the studies of exotic ethnography that had hitherto obliged me to make long and dangerous travels, and of concerning myself instead with our own people. I knew that Italy, though populated by folk of the same race, presented, especially owing to her history, a great variety of usages and customs; but the superficial examination of Samnite populations I could make in those few days convinced me that had I employed the best years of my life in the study of our own ethnography, I could have collected many objects and studied many customs now totally disappeared.

In 1906 Loria began to collect materials for a Museum of Italian ethnography; in 1910 he founded the Società Italiana di Etnografia; in 1911 he organized a large-scale exhibition of Italian folklore in Rome, the exhibits of which were to become the main body of the present Museo delle Arti e Tradizioni Popolari,[3] and sponsored the Jirst Congress of Italian Ethnography. The first number of *Lares*, to this day the leading folklore journal in Italy, was published under Loria's direction in 1912, hardly a year before his untimely death.

Folklore studies were by then already well developed in Italy, thanks to scholars such as A. D'Ancona, E. Rubieri, V. Imbriani, C. Nigra, A. De Gubernatis, and others (*cf.* Corso, 1946, *passim*); and especially in Sicily, where G. Pitrè (1841–1916) was completing the publication of a work of unparalleled magnitude (*Biblioteca delle Tradizioni Popolari Siciliane*, 1871–1913, 25 vols.), followed by the *Bibliografia delle Tradi-*

2. Most of his ethnographic collections are now in the Museo Nazionale Preistorico-Etnografico 'L. Pigorini,' Rome, along with his unpublished diary and field notes.

3. For many years in Tivoli, now in a new building in Rome EUR

zioni Popolari d'Italia, and he had also founded and edited a valuable journal, *Archivio per lo studio delle Tradizioni Popolari* (1880–1906). But Loria lamented that the interests of traditional folklore scholars were mainly literary in nature, limited to the recording of folk songs, proverbs, tales and the like, while old implements and artifacts received no attention and might very well disappear altogether before they had been recorded and collected. It was necessary, therefore, that folklorists should 'convert themselves into ethnographers' (Loria, 1912a, p. 19). Like most scientists of his generation, Loria was an evolutionist. He wrote (1912a, pp. 22–24), for instance:

> In the same way as one cannot study the history of Italy without being acquainted with the history of other nations, one cannot study the ethnography of Italy without being familiar with that of other peoples, whether they be civilized, semi-civilized, or savages. Among wide differences and chasms dividing one civilization from another, and all civilizations from barbarianism, there are connections and analogies which prove that the human soul has been, and is, always the same. General ethnography, just as it often serves palaeoethnological studies, can and must serve the study of our own people, because just as the savage shows similarities with primitive man, so our less evolved classes, lagging behind in the path of civilization, still preserve, hidden and slumbering, some of the instincts and characters of savage folk. ... Comparative ethnography, connecting our manners and customs with those of savage and semi-savage peoples, will be able to reach more general conclusions and to illustrate the genesis and phases of our own progress from time immemorial to our day. Then only will it be possible to accomplish the complex work of a truly scientific synthesis and the equally important task of education.

In his enthusiasm, Loria did not overlook the practical uses of the studies he was advocating. In a short paper published in the first issue of *Lares* (1912b, p. 78) under the significant title 'Ethnography as an instrument of internal and colonial policy', he wrote: 'If the knowledge of the manners and customs of the peoples subjected to a civilized nation assists the latter in preserving its rule, all the more so will the knowledge of manners and customs of our own people render unexpected services to our nation.'

Loria's insistent advice and his 'ethnographic' program were on the whole quite sensible, if not original; but unfortunately, there was no anthropological audience to receive and discuss them, still less to put them into action. In the account of the above-mentioned First Congress, given in the same issue of *Lares* by A. Mochi (1912, p. 25), one of the opening remarks is particularly significant:

> It was not, as is usually the case with scientific conferences, the meeting of specialists of a given discipline, assembled to discuss, so to speak, internal problems; it *was* a meeting of specialists indeed, but of specialists of the most varied disciplines ... classical scholars and physiologists, archaeologists and pedagogists, sociologists and physical anthropologists, philologists, glottologists, demographers, historians and jurists. ... True ethnographers are lacking or extremely rare in Italy.

The condition of the sciences of man in Italy on the eve of World War I can be summed up thus: A well-established tradition in prehistory and physical anthropology, promising developments in the field of folklore, with cultural anthropology (ethnology) not represented as an independent science in the academic set-up and considered rather as a subsidiary discipline to the other three.

'Historical' ethnology

The period between the two world wars was noticeable for two different trends: The influence of 'historical' ethnology on a few Italian scholars and a belated awakening of Italy's interest in colonial studies. The first aspect is no doubt connected to a certain extent with the fact that Italy was, and is, a 99 percent Catholic country; thus, the tenets and theories of evolutionism were met with suspicion or overt aversion among conservative students and orthodox believers. In these circles, opposition to biological evolutionism had been embarrassingly devoid of scientific arguments, but now the reaction against cultural evolutionism started by German geographers and museum curators seemed to offer new and sound weapons in the old controversy. P. Wilhelm Schmidt, who soon took the lead in the *Kulturkreis* movement, was summoned to Rome in connection with the great missionary exhibition of the 1925 Holy Year, which brought to the Vatican a conspicuous mass of ethnographic collections from all over the world, and was ordered by Pope Pius XI to display these collections at a new museum in the Lateran palace. The Museum was opened on December 21, 1927, and, though P. Schmidt, its first scientific director, soon passed on his post to his collaborators and did not stay on to teach in Roman universities, the man and his school exerted a lasting influence south of the Alps.

In 1937, on the tenth anniversary of the opening of the Lateran Museum, a new scientific journal appeared *(Annali Lateranensi)* under the editorship of Pater Schmidt. This journal was concerned primarily

with 'the cultural conditions of peoples in mission lands and first of all with their spiritual culture: religious, moral and social conditions, art, myths and legends with their respective texts' but also with their economic life, technology and linguistics. Significantly enough, physical anthropology is not mentioned in the program.

A few dedicated Italians who have since elected ethnology as their lifetime pursuit went to Vienna or to other German-language universities for their graduate or postgraduate studies, e.g., R. Boccassino, L. Vannicelli, and later V. Maconi and G. Guariglia. Their previous education had been in the humanities; none of them was a naturalist.

But strangely enough, an indirect and unacknowledged influence of the culture-historical trends was noticeable in scholars of the older generation, who had ostensibly taken a critical attitude toward the Vienna school and particularly the *Kulturkreis* theory, namely, R. Biasutti and R. Pettazzoni. Biasutti, the distinguished geographer of the University of Florence, was from the start a member of the editorial committee of the *Enciclopedia Italiana* and was responsible for the geographical and ethnological section. Though openly dissenting from the *Kulturkreislehre* on several of its positions and conclusions, he accepted the main methods of this school, especially as presented by the Swiss 'hologenist' G. Montandon; thus the *Enciclopedia* – one of the achievements of the Fascist period and in several respects a standard work to this day (original edition, 35 vols., 1929–1937) – offers most of its anthropological information in a mildly culture-historical light. In this and in his major treatise, *Razze e Popoli della Terra* (1st edition, 1941; 4th ed., posthumous, 1968), Biasutti was assisted by a number of similarly inclined collaborators, among them N. Puccioni, R. Battaglia and J. Imbelloni.

Pettazzoni, the historian of religions, had briefly been a curator of the Pigorini museum in his youth and was later a lecturer of ethnology at the University of Rome. An early and little known paper of his on African and Sardinian ordeals (1911) is interesting as a tacit rejection of evolutionary postulates; the comparison between the similar institutions in the two areas is based on the effort to indicate historical, not psychological, connections. Many of his works, especially *Miti e Leggende* (4 vols., Torino, 1948–1963) and *L'onniscienza di Dio* (Torino, 1955; English edition, *The all-knowing God*, London, 1956), bear witness to the extent and depth of his anthropological erudition. It is no doubt partly due to his influence that his pupils or followers, such as A. Brelich,

E. De Martino and U. Bianchi, have given primitive religions, and the associated anthropological facts and theories, such prominence in their writings and teaching in recent years. The last-mentioned scholar, now teaching ethnology as well as the history of religions at the University of Messina, has recently published a stimulating *Storia dell'Etnologia* (Rome, 1965). Yet another pupil of Pettazzoni's, V. Lanternari, has inaugurated the teaching of ethnology at the University of Bari with considerable success.

Last and not least of Pettazzoni's anthropological merits was the establishment of a postgraduate school, Scuola di Specializzazione in Scienze Etnologiche, at the University of Rome (1947).

Anthropology and the colonies

The catalyst provided anthropological studies by colonial rule needs no general illustration. For well-known historical reasons, Italy was, however, the last European state to acquire territorial possessions overseas. Her first settlements on the Red Sea coast, later to become Eritrea, date back to only 1885. These were followed in 1889 by others in present Somalia; Libya was conquered as late as 1911–1912. All these territories were economically poor, largely barren and sparsely populated–a liability more than an asset to the metropolitan state and unattractive for Italian settlers. Anthropologically, none of these colonies was populated by 'primitive' tribes in the commonly accepted sense; most of the population was either Moslem or Christian, with a literate tradition. These facts explain in part the small importance that anthropological studies proper acquired in the general frame of Italian colonial studies. A few fairly good ethnographic monographs, mainly by colonial officials, were produced over the years;[4] but significantly enough the major figures in the Africanist field were erudite orientalists, linguists and historians (F. Beguinot, C. Conti Rossini, M. M. Moreno, Enrico Cerulli), whose interest in anthropological matters was episodic and secondary.

The untimely and ill-advised conquest of Ethiopia in 1935–1936 caused an awakening of interest by both the authorities and the public; here at last was an immense territory of which at least the southern and western provinces were literally unexplored and which called for urgent

4. As fair examples, I should cite Ferrandi (1903) and Pollera (1913).

anthropological reconnaissance and mapping. Under the circumstances, hastily prepared overall surveys of territories and populations rather than meticulous and sophisticated analyses of societies prevailed. Under the sponsorship of the Royal Academy of Italy, the eminent geographer G. Dainelli, who many years before in 1913–1914 had participated in the great de Filippi expedition to Himalaya, Karakorum and Turkestan to study human conditions in those areas, directed the first full-scale scientific mission to the Lake Tana basin (*Missione di Studio al Lago Tana*, 1938–1943), and a zoologist, E. Zavattari, conducted two more expeditions to the Sagan-Omo region and to Borana country in southern Ethiopia. In these and other field research initiatives of those years, more attention was, as indicated, given to biological and physical-anthropological inquiries than to cultural ones. As things turned out, time proved too short for the completion of systematic research. V. L. Grottanelli planned ethnographic field expeditions to the almost unknown tribes of the Ethiopian west (*Missione Etnografica nel Uollega Occidentale*, 1940), but after an initial field trip in 1939 political events interrupted the plan, and it was finally abandoned.

The tragic events of the war not only brought practically all scientific research to a standstill but also acted negatively on Italian anthropology in a psychological sense by discrediting the utility of studies that rightly or wrongly the general public took to be connected with colonial policies and by discouraging incipient vocations. Among nonspecialists, the 'science of races' was understandably regarded with suspicion. It is all the more remarkable that in this generally unfavorable atmosphere, at a time when all links between Italy and her former African possessions were already severed (1941), a new scholarly journal should have been founded with the title *Rassegna di Studi Etiopici*. Directed at first by C. Conti Rossini, with V. L. Grottanelli and A. Mordini as editorial secretaries, and devoted to all branches of Ethiopian studies – linquistic, anthropological, historical and archaeological j– the journal managed to survive the crisis of the immediate postwar years. Its two successive editors after Conti-Rossini's death, M. M. Moreno and L. L. Ricci, have struggled to keep Ethiopian studies alive to this day.

Problems of terminology

Before we turn briefly to postwar developments, mention must be made of terminology. I have so far used the words 'anthropology' and 'anthropological' in the common meaning they have in English-speaking countries, but I have also mentioned, and must here stress, the fact that throughout the Italian tradition, in official academic terminology as well as in everyday usage, anthropology is understood to be the *biological* study of man (or, to use S. Sergi's definition, 'the natural history of the human family'); thus the adjective in the English expression 'physical anthropology' is superfluous in Italian. The Italian equivalent of American 'cultural anthropology' and (to a lesser extent) of British 'social anthropology' is simply *'etnologia'*. This clear-cut distinction, as I have shown above, was already firmly rooted and accepted almost without discussion when Italy's first academic society in this field, Società Italiana di Antropologia e di Etnologia (1870), was founded, and the terms have been challenged by only a limited group of scholars in recent years.

As a general term to indicate all the different branches of the study of man, 'anthropological sciences' is accepted in Italy, as it is in most other countries, though I am not sure there would be agreement concerning the range of disciplines that should be grouped under that label. Most of us would agree that it includes prehistoric archaeology (*Paletnologia* in Italian), folklore (seldom called *Demologia*, usually also *Storia delle Tradizioni Popolari* which is the name of this science in the official academic terminology), and of course ethnology and (physical) anthropology, and the occasional specialized branches of all these, but probably not psychology or linguistics and surely not classical archaeology or sociology.

These general groupings of the sciences of man need to have rather elastic, if not altogether ambiguous and vague, borders. The relevant fact I wish to emphasize is that at the level of research, theory and teaching, these sciences are (or have become) practically independent of one another. As a rule, ethnology and folklore are classed with the humanities; anthropology with the sciences (mathematical, physical, and natural); palaeoethnology with either faculty according to the set-up of the various universities. Faculties, however, being almost self-contained compartments, the practical consequence is that the hypothetical student interested in the whole range of the sciences of man experiences

some difficulty in obtaining an all-over grounding. Even in the post-graduate School of Ethnological Studies, most courses and exams being optional, students may get their degree without having been trained in such disciplines as physical anthropology, sociology, psychology or linguistics.

Though admittedly disappointing to partisans of the holistic and indivisible study of man, this *de facto* division has its roots in the distinction between *Naturwissenschaften* and *Geisteswissenschaften* accepted to this day, at least in practice, in the Italian academic tradition. Even the positivistic trends of the nineteenth century did not succeed in blurring that classic distinction.

Some institutions persist in the effort to bridge the gap, thus paying tribute to the nineteenth-century programs of integrated knowledge and action. From its foundation, the Istituto Italiano di Antropologia has held periodic meetings during which ethnological and/or psychological papers are read by members or guest lecturers in deliberate alternation with biometric ones, and so, to a lesser extent, do the Florence anthropological Society, and C.I.S.P. (Comitato Italiano per lo Studia dei Problemi della Popolazione). This latter society, founded in 1928 in Rome at the initiative of C. Gini, aimed at 'a comprehensive view of the many different aspects of a field covering all human sciences', and one of its statutory principles is 'to get specialists in other related fields to unite in the study of population problems' (where by 'other' is meant other than demography and statistics) (*cf.* Frederici, 1966).

S.I.P.S. (the Italian Association for the Advancement of Sciences) follows a similar trend whenever its meetings are devoted to the sciences of man. When the University of Turin organized an anthropological congress in 1961, in the framework of the centenary celebrations of Italian unity, it was agreed that the title should be 'Congresso di Scienze Antropologiche, Etnologiche e di Folklore' (*Atti del I Congresso*, 1963).

But probably the most interesting evidence of debate on the systematic no menclature of our sciences is the outcome of an international inquiry on the subject, promoted by the Società Romana di Antropologia in 1932 and published only in 1947 (Sergi, 1944–1947), the delay being due to the practical difficulty experienced in obtaining a satisfactory range of replies and subsequently to war and postwar conditions.

Four questions were circulated in writing by the society to leading anthropologists all over the world on the following points:

1. The meaning of the terms anthropology and ethnology.

2. The distinction between the terms ethnology and ethnography.
3. The limits of the anthropological sciences for theoretical purposes and for those of teaching.
4. The extent and position of the folkloristic research.

Answers came from 71 scholars, including F. Boas, J. Czekanowski, R. B. Dixon, E. von Eickstedt, W. Koppers, O. Menghin, R. Thurnwald, H. Vallois, F. Weidenreich, and were reproduced in full in the respective original languages, with a final comment by S. Sergi. The roster of significantly conflicting opinions is still, after several decades, provocative and instructive. However, as we are here concerned only with Italian positions, I shall limit myself to translating, in slightly abbreviated form, the answers given by R. Pettazzoni (Sergi, 1944–1947, p. 47) which come the closest to defining the prevailing Italian tradition:

1. 'Anthropology' in the specific sense is the study of man in the *physical* order. 'Ethnology' is the study of man in the *cultural* order.
2. Ethnography has a descriptive character, dealing with a single cultural unit. Ethnology has a comparative character, extending to different cultural units.
3. Anthropological sciences (palaeoanthropology, etc.) belong to the order of natural sciences. Ethnological sciences (palaeoethnology, prehistoric archaeology, etc.) belong to the order of historical sciences.
4. Folklore is ethnography/ethnology of survivals and subordinate cultural formations within modern civilizations. As such, folklore is an ethnological science.

Traditional positions and postwar trends

It is interesting to note that Pettazzoni's definitions correspond much more closely to those of W. Koppers and H. Weinert than to those of his Italian colleagues in the field of physical anthropology.

A few general considerations will help to clarify the present position of anthropology in Italy. Ethnology, as cultural anthropology is normally called in Italian, has retained 'primitive' societies and cultures as the main (in fact almost the only) object of study and culture history as its major trend. Structural-functional methods and theories have found practically no followers; psychological approaches have also met with little approval.

The number of ethnologists in Italy has always been relatively small compared to that of specialists in cognate disciplines such as physical anthropology, folklore and even prehistory. This may in part be explained by the fact that in the latter sciences, permanent University chairs (even if few) were available since the nineteenth century, whereas ethnology – though taught as an optional science in a dozen universities in recent years – obtained similar official recognition only as late as 1967. To put it bluntly, the study of exotic societies was an intellectual luxury bringing no reward in the way of a career, academic or other. A less direct reason may be the great attraction seen by students in a related and, in a sense, competitive discipline, archaeology, in a country where the cult, study and preservation of a grand indigenous tradition not only strongly appeals to the natives (as in Greece, Iran, etc.) but also offers appreciable career opportunities.[5]

It is a fact that Italians have so far shown little inclination and therefore contributed little to theoretical speculation in the field of cultural anthropology. This is probably due not to academic deficiency or to provincial isolation[6] but mostly to the general orientation already mentioned: If ethnology is understood as a special section of history, it calls for reconstruction of trends and events, interpretation, and search for cultural connections and origins, rather than for scientific hypotheses and abstract theories.

During the postwar years, however, a number of new developments have occurred, a few of which can be mentioned, though they cannot uniformly be classed as belonging to the national tradition or even in keeping with it. After what has been said above, the fact that individual contributions have continued, in recent years, to come from nonethnologists should no longer appear paradoxical. A few instances are the discovery and initial exploration of Andaman kitchen middens, fundamental for the unraveling of the islanders' past, the work of L. Cipriani (1955, p. 250ff.), a physical anthropologist; some of the most valuable contributions to the knowledge of African societies coming from R. C.

5. The executive and scientific personnel of the whole Italian Administration of Antiquities and Fine Arts, which is responsible for museums as well as for monuments, included in 1967 only four anthropologists, as opposed to some 170 archaeologists, art historians and architects.

6. Admittedly, Italian being beyond our frontiers an incomprehensible language even the little that has been and is being written on methodology and theory in this country is ignored by the overwhelming majority of scholars abroad, thus making a constructive dialogue impossible.

missionaries (*cf.* esp. Bernardi, 1959; Santandrea, 1964); a fascinating and unique document on a Tropical Forests culture – drawn, in her very words, from the life-history of a white woman who 'went native' and spent 20 years as a full member of a remote Indian tribe – due to E. Biocca (1966), a professor of biology.

Two of the most significant figures in Italian postwar ethnology who actively influenced younger generations in years of teaching were late recruits to the discipline: A. C. Blanc (1906–1960), a distinguished palaeontologist and prehistorian, who contributed stimulating hypotheses to culture history, in contrast to the usual assumptions of diffusionists (1940; *cf.* Vannicelli, 1953, pp. 22–27) and E. De Martino (1908–1965), a disciple of A. Omodeo and B. Croce, who made his debut with a sparkling attack on both 'historical' ethnological methods and on functionalism and who later applied anthropological field techniques to the study of archaic customs and beliefs in Southern Italy (1941, 1948, 1958, 1959, etc.).

The last-mentioned attempt reminds one of L. Loria's programs, but the resemblance is only superficial: Confronted with rapid culture change, Loria emphasized the urgency of preserving and collecting the elements threatened with disappearance, in view of their value as archaeological relics; De Martino was interested in the acculturation problem itself and even more in the awkward fate of the backward classes faced with it. The Loria line, on the other hand, has been continued not only by traditional folklore students but also by geographer-anthropologists like Biasutti, one of whose last achievements was the editing of a series of volumes on (disappearing?) rural houses in the various Italian regions (1938–1957).

As a rule, insofar as one can speak of consistent trends in Italian anthropology, the prevalent attitude has been a conservative one, in the sense that interests are still centered on illiterate societies overseas, leaving to folklorists the task of studying cultural survivals and ethnic processes among European peoples and to sociologists the problems arising from industrialization and urbanization of rural classes in modern countries. This division of labor is, of course, not always clear-cut, nor is it understood as a matter of principle but rather of expediency.

A different approach has been taken in recent years by a group of young scientists who have elected to call themselves cultural anthropologists *('antropologi culturali')*. They not only repudiate the established denomination of 'ethnologists' but also openly disavow their

allegiance to ethnology and all it stood for in the Italian tradition; indeed, they proclaim the existence of a distinction between ethnology and cultural anthropology in subject, scope and method.

The question of names was no trifle or matter of personal preference in a system like the Italian one in which all the important universities are state-run and in which no subject of teaching is conceivable unless it bears an officially accepted and registered name. One of the first labors of the separatists, and so far probably the most important one, was to ensure from state authorities recognition of what to all purposes was a new label. The initial step in this process, 'the institutionalization of cultural anthropology', was made in 1958, with the official birth of what was impartially called the Institute of Ethnology and Cultural Anthropology at the University of Perugia. (It had first been created in the academic year 1955–1956 as an Institute of Ethnology.) The first decree by which the teaching of cultural anthropology is inserted into a university statute (the Catholic University, Milan) was issued in 1961. Further steps followed rapidly, and by 1966–1967 cultural anthropology was listed among the graduate courses in eleven faculties, although it was actually taught in only seven. Until 1971, there was no permanent chair for this discipline.

Judging from a recent survey,[7] the variety of situations and problems which this new group is investigating, or proposing to investigate, is astounding, ranging from 'researches on the present socio-cultural dynamics in the rural world' to 'theoretical studies on (mental) alienation'; from inquiries on the 'psycho-cultural integration of urbanized individuals and immigrants in general' to studies on the 'roles and images of woman in ladies' periodicals'. As a concession (or a challenge?) to oldtime ethnologists, 'field researches on primitive communities' is included as a subitem in the list of the 18 or more tasks which the cultural anthropologist is entitled to pursue.

Though several of these tasks are in themselves commendable and possibly of practical utility, the program contains little that is original, owing to its obvious (and acknowledged) derivation from well-known American models and trends. Even on the more limited national scale–with reference to what is described as 'the attitude of new discovery of the country's [Italy's] social reality'–it is not altogether new, as can be

7. Seppilli (1967); *cf.* the special issue of *Bollettino delle Richerche Sociali* on the same subject and *De Homine*, 17–18: 3–457.

seen by looking back, for instance, at G. Arias's major work (1921) and its substantial bibliographical appendix (1921)[8] though things were called by less sophisticated names. Many of these 'new' social problems had been identified and stated decades ago and a study of solutions had been attempted.

At any rate, if it succeeds in asserting itself as an independent branch with in the general framework of the sciences of man, Italian cultural anthropology will probably have little in common with traditional Italian ethnology. It seems to be at the cross-roads of disciplines hitherto known in Italy a sociology and folklore, with an occasional sprinkling of psychology, statistics, demography, economics, etc. Its program pretends to such coverage that while it poses a question of scientific taxonomy, it also presupposes an interdisciplinary cooperation to a far larger extent than was the case with ethnology.

A broadening of interests is always welcome, the more so if it is to lead to an enrichment of scientific experience and knowledge beyond the limits of intellectual fashion or political convenience. That is the touchstone according to which any scientific experiment is in sufficient time incorporated and integrated into the living body of tradition–or rejected from it.

References

Aris, G. (1921), *La questione meridionale,* 2 vols. Bologna.

Atti del I Congresso, etc. (1963), Torino, Istituto e Museo di Antropologia dell 'Universita.

Atti della Society Romana di Antropologia (1893), vol. 1, fasc. 1. Roma.

Barocelli, P. (1937), *Il Regio Museo Preistorico-Etnografico 'Luigi Pigorini' di Roma.* Roma.

Bernardi, B. (1959), *The Mugwe: A failing prophet.* London, International African Institute.

Bidney, D. (1953), *Theoretical anthropology.* New York.

Biocca, E. (1966), *Yanoama.* Bari.

Blanc, A. C. (1940), *Etnolisi: sui fenomeni di segregazione in biologia ed in etnologia.* Roma.

Bollettino delle Richerche Sociali (1961), 1 (6). Bologna.

Carletti, F. (1958), *Ragionamenti del mio viaggio intorno al mondo.* Torino.

8. Arias (1921). Similar problems had been studied from an anthropological angle by A. Niceforo (1908).

Cavazzi da Montecuccoli, G. A. (1687), *Istorica descrizione dei tre regni Congo, Matamba et Angola.* Bologna.

Cipriani, L. (1955), Excavations in Andamanese kitchen-middens, *Actes du IV Congres des Sciences anthropologiques et ethnologiques,* 2: 250ff. Wien.

Corso, R. (1946), *Folklore.* Napoli.

Costantini, G. A. (1747), *La verita del Diluvio Universale vindicata dai dubbi.* Venezia.

De Martino, E. (1959), *Sud e magia.* Milano.

— (1958), *Morte a pianto rituale nel mondo antico.* Torino.

— (1938–1957), *Consiglio Nazionale delle Richerche, Richerche sulle dimore rurali in Italia,* 17 vols. Centro di Studi per la Geografia Etnologi ca presso l'Universita di Firenze.

— (1948), *Il mondo magico.* Torino.

— (1941), *Naturalismo e storicismo nell'etnologia.* Bari.

Ferrandi, U. (1903), *Lugh.* Roma.

Frederici, N. (1966), L'opera di Corrado Gini nell'ambito della demografia e delle scienze sociali, *Genus,* 22 (1–4): 7–41. Roma.

Gilij, F. S. (1780), *Saggio di storia americana o sia storia naturale, civile e sacra dei regni e delle province spagnole di terra ferma e nell'America meridionale.* Roma.

Grimaldi, F. A. (1779–1780), *Riflessioni sopra l'ineguaglianza fra gli uomini.* Napoli.

De Homine (1966), 17–18: 3–457. Roma, devant De Martino page précédent.

Loria, L. (1912a), Due parole di programma, *Lares,* 1: 9. Roma.

— (1912b), L'Etnografia strumento di politica interna e coloniale, *Lares,* 1, 78. Roma.

Massaia, G. (1885–1895), *I miei trentacinque anni di missione nell'Alta Etiopia,* 12 vols. Roma.

Missione Etnografica nel Uollega Occidentale (1940), vol. 1. Roma, Reale Accademia d'Italia.

Missione di Studio al Lago Tana (1938–1943), 6 vols. Roma, Reale Accademia d'Italia.

Mochi, A. (1912), Il Primo Congresso d'Etnografia Italiana, *Lares,* 1: 25. Roma.

Niceforo, A. (1908), *Antropologia delle classi povere.* Milano.

Penniman, T. K. (1935), *A hundred years of anthropology.* London.

Pettazzoni, R. (1911), Ordalie sarde e ordalie africane, *Rivista Italiana di Sociologia,* 1: 36–45.

Pollera, A. (1913), *I Baria, i Cunama e i Beni Amer.* Roma.

Ricci, Matteo (1911–1913), *Opere storiche.* P. Tacchi-Venturi (Ed.). Macerata.

Santandrea, S. (1964), *A tribal history of the western Bahr el-Ghazal.* Bologna, Museum Combonianum n. 17.

Sassetti, F. (1961), *Lettere indiane.* Torino.

Seppilli, T. (1967), *Rapporto sulla antropologia culturale.* Milano, Centro Nazionale di Prevenzione e Difesa Sociale.

Sergi, S. (1944–1947), Terminologia e divisione delle scienze dell'uomo: i risultati di un'inchiesta internazionale, *Rivista di Antropologia,* 35: 5–83. Roma.

Vannicelli, L. (1953), *I recenti contributi e indirizzi etnologici in Italia.* Bologna.

Vico, G. B. (1725), *Principi d'una scienza nuova d'intorno alla natura delle nazioni.* Napoli.

10 The Netherlands: Structuralism before Lévi-Strauss

PATRICK EDWARD DE JOSSELIN DE JONG

My aim in this paper is to discuss the national tradition of cultural anthropology in the Netherlands mainly as a creature of circumstance: to consider how it has been molded and remolded by extraneous factors such as the academic environment and the wider political context in which it developed. I deliberately refrain from treating it as part of certain scholarly traditions or currents of thought prevalent in its home country (or Western Europe generally) in various periods. I realize that this approach has a certain inherent shallowness, but at this stage it seems preferable to first get the record straight and to outline the more concrete, objectively discernible factors.

Cultural anthropology – as it is usually called in the Netherlands, rather than social anthropology – has had a rather curious history in that country. First, I should note that, in spite of its American-sounding name, it follows the European rather than the American tradition in the relative narrowness of its scope. Although periodicals like *American Anthropologist* and *Current Anthropology* also deal with physical anthropology, prehistory and archeology, and linguistics, in Holland these fields have firmly, perhaps rather too firmly, been set aside from cultural anthropology and are considered as completely independent disciplines. This is not surprising as much the same situation prevails in Britain. The remarkable thing, however, is that several topics which we all *do* consider as properly anthropological, were until very recently also separated from the main stem of cultural or social anthropology.

A person specially interested in so-called 'primitive' or customary law, for example, was not considered to be specializing in a particular field within social anthropology but was seen as a different kind of

scholar altogether; he was dealing with *'Adatrecht'*, customary law, a totally independent discipline. Until a few years ago the Chair of Customary Law in Leiden was in the Faculty of Law; the Chair of Cultural Anthropology, in the Faculty of Arts. It was only in 1963 that both found a common meeting place in the Faculty of the Social Sciences. The same holds true for 'primitive' economics. Again, it was not regarded as a specialization within anthropology but rather as a separate discipline–a specialization of anything in general economics. Moreover, there was until recently the tendency to link the study of the complex societies of Asia and Africa with language and literature studies rather than with anthropology. If you were interested in modern Chinese society, for instance, you would not become a sociologist or anthropologist who acquires enough knowledge of Chinese for his purpose; no, you would become a student of Chinese, of classical Chinese mainly, who might be allowed to dabble in the sociology of China in odd moments. In the same way the study of Muslim society was encapsulated in the study of Arabic – classical Arabic – and of Indian society in Sanskrit studies. It has only been during the last eight years or so that Dutch scholars have been going to India to study Vedic ritual as it is practiced *now*, or land reform or the caste system, as it is *now*.

There was a reason for this apparently unreasonable state of affairs. When, in the last century in Holland as elsewhere, a scholarly interest arose in actual living societies, this interest understandably enough concentrated itself *on the societies of Holland's vast colonial empire in the East Indies*. On the one hand, this resulted in a concentration of anthropological study on Indonesia; the study of China was left to sinologists, the study of the Muslim world to Arabic scholars, etc., and Africa was neglected altogether. On the other hand, the anthropological or ethnological study of Indonesian cultures themselves at a university level was set up on a purely pragmatic basis; it was a subject in the university's training course for colonial Civil Service trainees. This explains why the study of customary law was split off from anthropology; it was a subject not for prospective colonial administrative officers but for trainees of the colonial legal service, who followed a different training course at the university.

So also for 'primitive', i.e. native Indonesian, economy. This was seen as the counterpart to the Western economy in the allegedly dualistic economic structure of the Netherlands East Indies. The practical problem in colonial administration raised by this dual economy was considered

a fit subject for economists but not for ethnologists. And once we accept the pragmatic approach, there is a lot to be said for this point of view.

Thus we find cultural anthropology starting its career in Holland as a university discipline with the purpose of giving practical, useful knowledge of Indonesian peoples to prospective colonial administrators. This began at a college not in Holland itself but at Surakarta in Java, where as early as 1834 the curriculum included the subject 'Native Institutions of Java'. In 1877 this was transferred to Leiden University, where a chair was established in the 'Geography and Ethnology of the East Indian Archipelago'.

This pragmatic rather than scholarly approach was apparent in the linking of ethnology with geography, and the first occupant of this chair, P. J. Veth, proved by his person that at this time one was interested more in 'knowing a lot about' Indonesia than about 'ethnology'. Veth passed from theology to Semitic languages. He became interested in Arabic, via Arabic in Islam, via Islam in the Muslims of Indonesia, and, via the Muslims, in Indonesian societies in general. His best-known book (1896–1907), *Java geographisch, ethnologisch, historisch (Java, its geography, ethnology, and history)*, is even more encyclopedic than the designation of his university chair.

His successor, Wilken, was an exceptional figure, firmly placing the study of Indonesian societies in the context of the dominant ethnological theory of his time, evolutionism. Among the Dutch ethnologists of his time, he was, unfortunately, unique in that he played a major role in the exchange of ideas with his prominent contemporaries Tylor, Frazer and Robertson Smith. But in his successors we see once more that when it came to teaching and studying the 'Ethnology of the East Indian Archipelago', as it was called, knowledge of the East Indian Archipelago was held to be more essential than knowledge of ethnology.

His first successor was De Groot, still deservedly well known as a sinologist, but not as an anthropologist. He was appointed on the basis of his undoubtedly competent research on the Chinese, particularly Chinese secret societies, in the Netherlands Indies. Early in this century the name of the chair was changed to 'The History, Literature, Manners and Customs of the Peoples of the East Indian Archipelago, and the Physical Geography of that Region'. Chosen to succeed De Groot in this impossibly broad appointment was G. W. Nieuwenhuis, a surgeon in the Dutch East Indies Army. Nieuwenhuis had participated in three

scientific expeditions in Borneo and had presented the results in several volumes crammed with ethnographical details (Nieuwenhuis, 1904–1907). As a disseminator of factual information about a part of Indonesia he did good work, but, again, he did not, and could not have been expected to, further the cause of anthropology in his country.

Things took a decisive turn for the better with the appointment of J. P. B. de Josselin de Jong in 1922, who was professor in Leiden until 1956. His subject was defined as 'The Cultural Anthropology of Indonesia and its Connection with General Anthropology'. This change of name was significant and realistic, for now at last the subject was in the hands of a man who was in the main stream of anthropological thinking. He maintained close contacts with his colleagues in the U.S.A., Britain, Germany and France and carried out, and trained his pupils for, field work in North America, the West Indies and Indonesia.

With him, the 'national tradition' takes a firm shape. Before we examine this more closely, a few more words about the situation of anthropology in the Netherlands in his period–the 1920s to the 1950s. Although university training in anthropology was now at the desirable standard, courses in anthropology still did not lead to a degree in anthropology; students were still destined to become colonial civil sevants, prospective members of the colonial magistrature or students of Indonesian languages or archeology. This had both advantages and disadvantages. The disadvantage is obvious: With this splendid, readily accessible, field for study, Indonesia, neither of the universities with departments of Indonesian studies – Utrecht and Leiden – turned out full-time '100%' anthropologists. Anthropological research remained a spare-time hobby of administrators, lawyers, missionaries, etc., who often had had *some* anthropological training at their university. There was, however, also a favorable side to this, namely, that anthropology did not remain in isolation but was closely linked with the other subjects in the department of Indonesian studies. Consequently, anthropology came to be closely bound up with Islamic studies, Indonesian philology and archaeology – not merely as a matter of organization but, more importantly, in its whole approach. Those who occupied themselves with the anthropological study of Indonesian societies were, from the beginning, trained to take into account data from oral or written literature and mythology and even historical or archaeological materials. These data were studied not for historical or pseudohistorical reconstruction, but fo their possible relevance to the *present-day* situation.

When Evans-Pritchard, writing in 1950, predicted that social anthropology would 'become, in a very general sense, the counter-part of Oriental Studies, in so far as these are conceived of as primarily linguistic and literary', he was describing the actual state of affairs of anthropology in Holland. One must not exaggerate; not all work was of this type, but much of it, and much that is most characteristic, was. It is interesting to observe how the scholarly character of a discipline in a particular country (which we can now consider as a national tradition in the practice of that discipline) here proves to be molded to a large extent by such a prosaic influence as the position of the subject in the university curriculum. We shall return to this point later on.

I should also note, in passing, a remarkable consequence of this state of affairs, which is causing us a lot of trouble at present. Dutch anthropological writings were, as we have seen, embedded in the context of Indonesian studies, both historical and philological. They were written neither by nor for out-and-out anthropologists but rather for Indonesianists of different persuasions who, however, knew Indonesia through both a common university background and their careers in that country. At present there are two committees at work in Holland issuing translations (mostly in English) of the best prewar Dutch publications on Indonesian sociology and anthropology. In these committees we are faced time and again with the problem that such publications are not made internationally accessible or comprehensible by translation alone because they were written for a closed group of readers with a specialist's knowledge of Indonesia which the present, wider, reading public lacks. The prewar publications always plunge headlong into details, as they take the reader's general background knowledge for granted. But we, translators of the 1960s, cannot take it for granted. We still have not found a solution to this dilemma.

Now let us try to indicate some general trends in these Indonesianist anthropological studies during the period when they were dominated by J. P. B. de Josselin de Jong. We will bear in mind that they are not armchair works but are based on observations in the field – not, however, by anthropologists, but by administrators, missionaries and others who usually had had some university training in anthropology.

In 1918 a magistrate, Van Ossenbruggen, published a long article which was to prove exemplary. In it, he discussed the Javanese system whereby a village unites with four other villages which are grouped around it in approximate accordance with the four main points of the compass.

Such leagues of four villages grouped around a fifth in the center deal with criminal cases and the maintenance of law and order generally. Van Ossenbruggen was struck by two facts: First, that similar arrange ments are also to be found elsewhere in Indonesia and, second, that in Java itself, such leagues of five often expanded by taking in a second, wider circle of four surrounding villages, and sometimes even a third, fourth or fifth circle: always, though, based on one central village, with ever-larger rings of four around it. This must be more than a coincidence, and the author found a key to its significance in Durkheim and Mauss's article 'Essai sur quelques formes primitives de classification'. In this publication the French ethnologists demonstrate that in various Australian tribes, the social divisions into moieties and clans correspond with a classification of birds, animals, plants and many other features of the universe.

Van Ossenbruggen applied this concept of classification to the Javanese village leagues of four-plus-one, eight-plus-one, etc. His task was easier than that of Durkheim and Mauss because Javanese have an elaborate system of astrology, numerology and divination based on the four cardinal points and their subdivisions. We need not follow Van Ossenbruggen's argument in greater detail. For our purpose, it is sufficient to note that he introduced the Durkheimian principle of classification into Indonesian anthropology and that this approach was welcomed by the Dutch professional or part-time anthropologists after him. On the one hand, it led to an interest in the more formalized types of social structure, based on moieties and unilineal descent groups; on the other hand, it led to studies of the way the native participants themselves associate these social groups with concepts such as upperworld and underworld, male and female, inland and coast, etc.

This interest in formal kinship structure also involved a study of what in Britain and France one usually calls 'alliance' or 'échange' and in Holland 'connubium'. In this way, a number of studies in the mid-1930s revealed what Leach (1951) was later to call 'the structural implications of matrilateral cross-cousin marriage', namely, asymmetrical connubium. This kind of connubial, or alliance, chain raises problems about the relation between the asymmetrical principle of the alliance groups and the apparently symmetrical relation between moieties, a problem later tackled by Lévi-Strauss in his article 'Do dual organisations exist?' (1956). The connubial relationships also involve exchanges of gifts, and in many Indonesian societies one finds that the goods given by the wife-

giving group are called 'female goods' or are given a specific name, e.g., 'cloth', whereas the counter-prestations of the wife-takers are given a general name, 'male goods', or a specific one, e.g., 'knives'.

This leads us once more to the relationship between social organization and symbol systems or classification. I would like to illustrate this topic by summarizing one more publication. It is a highly characteristic article, written in 1949 by Onvlee, which bears one of the most unusual titles ever given to an essay in social anthropology, namely, 'The barrage dam at Mangili'.

Just before the war, the Netherlands East Indies government built a large barrage dam in the Mangili district of the island of Sumba. This prompted Onvlee to make a study of the native system of dams and irrigation channels. He found that there was an irrigation channel leading off from the principal river. At a certain point this channel bifurcated into two wooden conduits, each leading to the rice fields of one of the principal exogamous patriclans of the district (Maru and Watu Bulu). Each conduit was also associated with one of these clans. The higher, i.e., upstream, conduit is called the male conduit; the downstream one, the female, as shown in Figure 1.

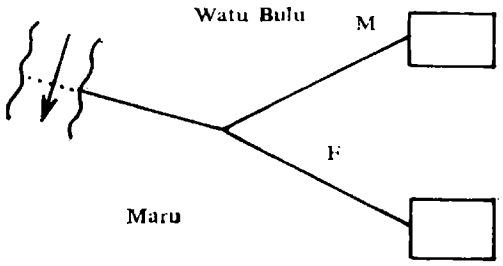

Fig. 1.

In poetic speech, the rice fields and conduits are likened to gold ear pendants and the chains attached to them. The pendants and the rice fields are said to be female, the chains and the conduits male (Fig. 2). The female conduit does not lead directly to its own rice fields but is laid out in such a way that it twice crosses the male conduit by means of an aquaduct. At these places the male and female conduits are said to live together as husband and wife.

The conduits are made of wood, which must be renewed from time to time. If it is the 'female' conduit that needs repair, the clan to which it belongs has to send emissaries to the other clan to ask for suitable

logs. This is done with the same formalities as are employed when the bride-taking clan approaches its regular bride-givers to request a wife for one of its clansmen. The same gifts are even exchanged. At the first

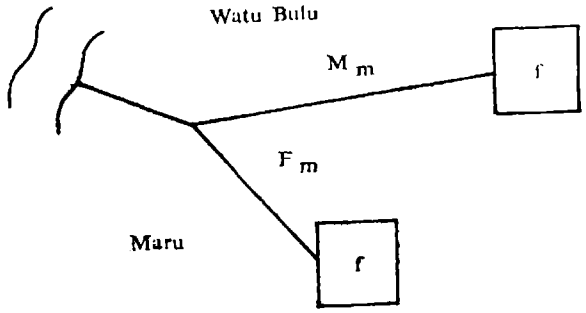

Fig. 2.

negotiations, the wife-receivers give gold ornaments and a horse and take a cloth back with them. Later, when a party of woodcutters of the first clan (Maru) enters the territory of the other to fell the tree for their conduit, they give a stallion and a mare and receive four more cloths. Finally, when the log is carried to Maru territory, the party that fetches the log, and which is still indicated as bride-takers, gives ornaments and a horse, while Watu Bulu gives two more cloths, 'to accompany the bride on her journey'. The parallel between Maru, the receivers of the log, and Maru, the wife-taking clan, is carried even further. The two places where the male and the female conduits cross each other, i.e. where they 'marry', may not be seen by members of the clan which in its turn is in wife-taking relationship to Maru (Fig. 3).

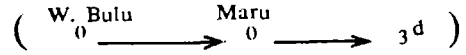

Fig. 3.

If a man of this third clan unavoidably has to approach these tabooed places, he must veil his face in passing. This is in accordance with the avoidance a man has to practice toward his wife's brother's wife; and to a member of this third clan, the log from Watu Bulu stands in this position.

I hope this summary gives an impression of the kind of studies the Dutch Indonesianists were especially interested in and which had become, and to a considerable extent remain, a Netherlands tradition in anthropology – the relation between unilineal kinship groups and their alliance

system on the one hand, with social symbolism and the native participants' classification system on the other. We see, by the way, how we are confronted here by a triadic and a dyadic system, closely intertwined.

Just a few more words on the dyadic classification. It may have struck you that we are not dealing with a simple male-female opposition, but with a more complex situation. The conduits, as opposed to the rice fields, are male, but one of the conduits is also male, as opposed to the female conduit:

Maru	*Watu Bulu*
F m	M m

The gifts exchanged are also in a general sense male (horses) versus female (cloths), but again with certain reservations; the horses received by Watu Bulu are expressly stated to be a stallion and a mare, whereas the cloths given to Maru are a male shouldercloth and a female skirt:

	Maru		*Watu Bulu*
conduit	F m	conduit	M m
cloths	F f	horses	M f
	F m		M m

The male element has a female admixture, and vice versa.

May we associate this with the fact that, although the Mangili clans are patrilineal, the traditional, orally transmitted genealogies (which may comprise more than ten generations) list individuals according to matrilineal descent? (Nooteboom, 1940, p. 22). In other words, although Mangili (and eastern Sumba in general) does not have true double descent, there are indications that one does reckon with both descent principles. A much oversimplified and crude model of a dual male/female, opposition in such a society could be as shown in Figure 4.

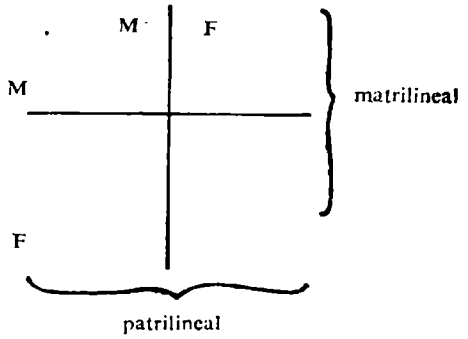

Fig. 4.

This also gives us the combination of male and female elements shown in Figure 5.

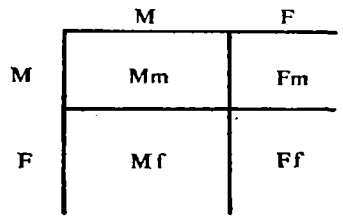

Fig. 5.

Further field work would be required to ascertain whether this model is applicable to real societies in Sumba and elsewhere in Indonesia. This is one of the problems Dutch anthropologists were interested in before and immediately after the war, when events made further research impossible. First the war and the Japanese invasion of Indonesia, then the deterioration and finally the complete rupture of relations between Holland and Indonesia brought an end to Dutch studies of Indonesian societies in the field, just when they were getting into their stride.

In recent years, diplomatic and academic intercourse between the two countries has resumed, and I foresee that one of the tasks of Dutch anthropology will be a revaluation of its own prewar and early postwar work in Indonesia. However, this is no longer a 'tradition' but a possible future trend. Recently, a British anthropologist wrote: 'A detailed survey of the work of the Leiden school is a task of considerable importance in the history of social anthropology, though not to be undertaken here.' I shall say amen to that and simply note that in the 1950s, political developments forced to a conclusion one chapter in that history.

As it became harder to gain admittance to Indonesia, the younger generation of Dutch anthropologists began to turn to an area which so far had been neglected, i.e., Western New Guinea. Here they were confronted with societies quite different from those which had claimed the attention of their predecessors. They were small, sometimes extremely small communities; a book published in 1956, for example, deals with two communities, each of about 70 individuals (Van der Leeden, 1956).

These microsocieties are faced with demographic problems, as they are continually faced with the danger of extinction, with little chance of recruitment from outside, because of intercommunal hostility. But in addition they live in a very hard natural environment. It is obvious that no elaborate, ideologically based unilineal or double-descent groups

will be found here, as they had been in Indonesia. On the contrary, what struck workers in this region was the continual process of adaptation; in kinship, in bride-price prestations, in leadership and succession to the headmanship, in agricultural techniques and economy, the picture was one of a series of rapid adjustments to *ad hoc* problems. One might almost say that these societies were in a state of continuous experimentation. This also implies a lack of time depth, compared with Indonesian societies. The latter have a great sense of historical continuity, which drew the attention of anthropologists and, interestingly enough, was fostered by anthropological studies. Partly through their anthropological training, the Dutch colonial administrators before the war had an emotional distrust of culture change. The result was a circle (must we call it a vicious circle?) involving historically minded societies, historicizing anthropological studies and literally *conservative* applied anthropology in administration.

In New Guinea the societies were different, as was the political situation, in that Dutch policy now aimed at modernizing Western New Guinea as rapidly as possible in order to make it capable of self-government. The consequence for the role of anthropology was a much greater interest in social change and acculturation–and, of course, in anti-acculturative movements such as cargo cults.

It is worth noting that the Dutch anthropologists (and by now in our narrative, the 1950s, we are dealing with full-time anthropologists as well as with anthropologically trained magistrates, etc.) were forced by circumstances to study societies lacking the familiar clan and connubium organization, just at a time when, internationally, anthropologists were beginning to pay more and more attention to nonunilineal descent systems (e.g., Goodenough, 1955, and Davenport, 1959, in America, Freeman, 1955, and Geddes, 1954, in Britain) and were taking renewed interest in ambilateral descent described before the war by Raymond Firth (*cf.* Firth, 1949).

Both the force of circumstance and the development within the discipline itself modified the national tradition of anthropological research by stimulating work on small-scale nonunilineal social systems in New Guinea, and quite a number of works on them appeared in rapid succession. Most of them were written in Dutch, unfortunately, but we hope that several will soon be translated into English.

The theoretical consequences of the switch-over from Indonesia-centered to New Guinea-centered anthropology was illustrated rather

amusingly when Dr. Pouwer, trained in the Leiden tradition of Indonesianist studies, was appointed professor of anthropology at Amsterdam University. His inaugural lecture (1963) was a plea for concentrating less on the formal aspects of social *structures* and more on the role of individual choice and decision in social life. You can imagine the resulting storm in the anthropological teacup.

This kind of controversy could have led to a most useful cross-fertilization between anthropologists of the pre- and post-war traditions if politics had not once more intervened. Under United Nations supervision, the administration of Western New Guinea was transferred to Indonesia until 1969; meanwhile, the region became almost inaccessible to anthropologists, certainly to those of Dutch nationality. Once more this led to a shift of focus, this time to an area with which Holland maintains a special relationship – Surinam (formerly also known as Dutch Guyana), on the north coast of South America.

Here again, the area one works in dictates to some extent the kinds of problems one shall have to study. The total population of Surinam is about 200,000. Of this population, the aboriginal component, the South American Indian tribes, forms a minority. The largest group is made up of descendents of the former slave laborers on the plantations, of West African origin. This African element is now to be found as city dwellers and as forest creoles (to use the local terminology). Other large immigrant groups are Javanese, Indians from Uttar Pradesh (called Hindustanis in Surinam) and Chinese.

It is understandable that in such a country the social anthropologist will, almost unavoidably, devote much attention to interethnic relations, including ethnic prejudice and stereotypes, to the question of whether Surinam is a 'plural society' and to the development of the typically creole culture. In 1963 a special issue of *Bijdragen*, the Dutch linguistic and anthropological quarterly, was devoted to Surinam, and it contained the following articles: 'Surinam slaves and free negroes in Amsterdam in the 18th century', 'Geographical mobility as a factor in the development of Surinam', 'Change in prejudice: Some notes on the minority problem, with references to the West Indies and Latin America', 'The attitude of the Hindustani population in Surinam towards the Creoles', 'The attitude of the Creole population of Surinam, in particular towards the Hindustanis', 'The financing of the tayub, a Javanese feast of merit in Surinam' and 'New perspectives in the study of acculturation'. This catalog is fairly representative of the kind of

work that is at present attracting quite number of the younger Dutch anthropologists.

At the congress of anthropology and ethnology held in Paris in 1960, Professor Maquet (1963) read a very well-reasoned paper in which he demonstrated that the dominant anthropological interests in the pre-1914 period, in the 1920s and 1930s and in the 1950s were, as he put it, culturally conditioned by the views of Western society as a whole as to what is (or was) the most desirable relationship between the Western and the non-Western worlds.

My present paper leads to a rather similar conclusion: We see that the nature of the Netherlands' national tradition in cultural anthropology and its development have been determined to a great extent by the kind of societies which were most easily accessible to Dutch anthropologists: in Indonesia, unilineal descent groups, connubium and classification systems; in New Guinea, cognatic systems and the adaptation of small, loosely structured societies to a hard, and at times marginal, demographic and natural situation; in Surinam, interethnic relations and acculturation. It is this interplay between mundane, including imperial, affairs and academic theory which I think makes the national tradition, and its modifications, of anthropology in Holland a rather interesting case. When we discuss *it*, we are dealing with the sociology of knowledge; we are not merely recording the thoughts and acts of an aberrant group of herring fishers and cabbage growers of the Rhine delta. We dealt with three regional specializations: Indonesia, New Guinea and Surinam, each with their concomitant thematic interests.

I would summarize the present-day situation in each of these fields as follows: Relations are being cautiously resumed with Indonesia after an interval of several years; several Dutch anthropologists are or have been once more doing field work there; the topic so closely linked with Indonesianist studies, namely, classification systems, still occupies our attention, particularly at Leiden University. It is understandable that the work of Lévi-Strauss has from the very beginning proved most congenial to this group.

Dutch field work in New Guinea has come to an end. The results of some research carried out before 1961 still remain to be written up and published. Many efforts are now being concentrated on Surinam and the topics of interethnic relations, acculturation and urbanization. There is a tendency to fan out from Surinam, and field work on these subjects is in progress or planned elsewhere in the Caribbean.

In addition, for the first time, anthropologists from Holland are making their way into regions formerly *terra incognita* to them. Amsterdam University has successfully started quite an ambitious project for the study of land reform and other topics in Gujarat (Northwest India), and the Institute of African Studies (in which several universities cooperate) has embarked on a pilot project for the study of agricultural economy in East Africa. All this is still exploratory, as we lack the tradition, the contacts and the background information such as have been painstakingly built up by anthropologists in Britain, for example.

I should enter one caveat before concluding. Several features of the 'Netherlands tradition' in cultural anthropology outlined above, and in particular the structural approach, originated at and are still characteristic of especially, though by no means exclusively, Leiden University. (A French colleague once referred jokingly to *le mouvement structuraliste Paris–Leyde.*) It may therefore seem as if I have arrogantly raised a hobby of my own alma mater to the status of 'national tradition' of Netherlands anthropology as a whole. This is by no means my intention. Even in a small country like Holland, there has always been a considerable diversification of interests among the various universities' anthropology departments. For example, Nijmegen University maintained, and to a certain extent still does maintain, close ties with the German-Austrian *Kulturhistorische* school. In Amsterdam, the noted sociologist-ethnologist Steinmetz early in this century initiated a large-scale comparativist approach, rather similar in principle to the present-day HRAF procedure. Present-day Amsterdam anthropologists have carried forward Steinmetz's interest in an up-to-date form for what they call their 'comparative-functionalist method' (*cf.* Köbben, 1952, 1955). Nevertheless, it is my considered opinion that it is the 'structuralist' approach and its derivatives which show the longest and most consistent line of development. This approach has set a special stamp on the work of a large and considerable group of Netherlands anthropologists, and it may therefore be most fittingly said to represent a 'national tradition'. It would be foolish to claim that the work done by anthropologists of this 'school' is better than that done by their compatriots of other persuasions, but I do claim it is the most characteristic.

At present, social anthropology in the Netherlands is obviously in a period of transition. As it diversifies its regional specialization, it also loses its traditional idiosyncratic features. For better or for worse, it

is becoming the willing victim of an acculturation process emanating from the centers with which it as a discipline and Holland as a country maintain such pleasant contacts–France, the British Commonwealth and the U.S.A.

References

Davenport, W. (1959), Nonunilineal descent and descent groups, *American Anthropologist,* 61.

Durkheim, E., and Mauss, M. (1901–1902), Essai sur quelques formes primitives de classification, *L'Annee Sociologique,* 6.

Evans-Pritchard, E. E. (1950), Social anthropology: Past and present, *Man,* 50.

Firth, R. (1949), *Primitive economics of the New Zealand Maori.* London.

Freeman, J. D. (1955), *Iban agriculture.* London.

Geddes, W. R. (1954), *The hand Dayaks of Sarawak.* London.

Goodenough, W. (1955), A problem of Malayo-Polynesian social organization, *American Anthropologist,* 57.

Köbben, A. J. F. (1955), *De vergelijkend-functionele methode in de Volkenkunde.* Groningen, Djakarta.

— (1952), New ways of presenting an old idea: The statistical method in anthropology, *Journal of the Royal Anthropological Institute,* 82 (2).

Leach, E. R. (1951), The structural implications of matrilateral cross-cousin marriage, *Journal of the Royal Anthropological Institute,* 81.

Leeden, A. C. van der (1956), *Hoofdtrekken der sociale structuur in het westelijk binnenland van Sarmi.* Leiden.

Lévi-Strauss, C. (1956), Les organisations dualistes existent-elles?, *Bijdragen Koninklijk Instituut,* 112.

Maquet, J. (1963), Le conditionnement social de l'anthropologie culturelle, *VIe Congrès International des Sciences Anthropologiques et Ethnologiques,* vol 2. Paris.

Nieuwenhuis, A. W. (1904–1907), *Quer durch Borneo,* 2 vols. Leiden.

Nooteboom, C. (1940), *Oost-Soemba.* The Hague.

Onvlee, L. (1949), Naar aanleiding van de stuwdam in Mangili, *Bijdragen Koninklijk Instituut,* 105.

Ossenbruggen, F. D. E. van (1918), De oorsprong van het Javaansche begrip montja-pat, in verband met primitieve classificaties, *Verhandelingen Koninklijke Akademie van Wetenschappen,* Afd. Letterk., vol. 5–3.

Pouwer, J. (1962), *Het individu in samenleving en cultur.* Groningen.

Veth, P. J. (1896–1907), *Java, geographisch, ethnologisch, historisch,* 4 vols. Haarlem.

11 Scandinavia: All approaches are fruitful

JOHANNES NICOLAISEN

The American concept of anthropology as a complex of integrated sciences including cultural and/or social anthropology, physical anthropology, archaeology and linguistics has not yet been introduced into Scandinavia. In the Scandinavian countries these branches of human science normally have their independent chairs and institutes in the universities. Consequently, there is often very little cooperation among these anthropological disciplines, which have no uniform and common goal. Recently, initiative has been taken in Stockholm to use the word 'anthropology' in its American meaning and to integrate the various anthropological disciplines by establishing 'The Nordic Council for Anthropological Research' (Hultkrantz, 1915). The membership of this institution comprises anthropologists from Denmark, Norway, Sweden and Finland and covers the fields of cultural/social anthropology (ethnology), Nordic folklife research, folklore, archaeology and linguistics. Physical anthropology, though, has not yet been included. This may seem remarkable in view of the fact that, properly, the Scandinavian term *'antropologi'* means 'physical anthropology'.

In this paper I use the term 'anthropology' mainly for cultural/social anthropology, although this is contrary to Danish usage. In Denmark cultural/social anthropology is called ethnology *('etnologi')* or ethnography *('etnografi')*. As in German-speaking countries, there is a tendency to use *'etnologi'* when speaking of theoretical and comparative analysis and to reserve *'etnografi'* for descriptive accounts of the culture of particular peoples or regions. However, there is also a marked tendency to use *'etnologi'* for studies dealing with European folk cultures and *'etnografi'* for studies of so-called primitive cultures. This holds true in particular for Norway and Sweden.

Danish anthropology

Danish anthropological writing is of long standing and dates back to the Age of Enlightenment. It is now more than two centuries ago that the Dano-Norwegian scholar Jens Kraft (1720–1765) wrote his *Kort Fortaelning af de Vilde Folks fornemmeste Indretninger, Skikke og Meninger* (i.e., *Brief account of the principal institutions, customs and ideas of the savage peoples*). This interesting book contained 383 small pages and was published in Danish in 1760 at the Danish provincial town of Sore. Birket-Smith considers this work the first general ethnology in any country and states that it should still be considered a milestone in the history of science, as in many respects it was far ahead of its time. Kraft's early anthropological ideas have been outlined by Birket-Smith (no date) and there will be no need for an elaborate account in this paper. It should be emphasized, however, that though Kraft belonged to the Age of Enlightenment, he did not occupy himself with that elementary concept of environmentalism which was so typical of many great writers of that period. Kraft had a rational, independent mind and possessed great knowledge of the existing literature on exotic peoples. Some of his general ideas deserve to be mentioned in brief:

1. Primeval man did not live in a state of promiscuity, as even among the higher animals we find something like marriage–the argument of Westermarck 130 years later.

2. Matrilineal succession is due to the fact 'that the children have for the most part followed their mothers and considered themselves their natural possessions' (*cf.* Bachofen's discovery a century later).

3. Cannibalism is not merely a rapacious custom; it is rooted in religious ideas.

4. The tools of early man were made of wood and in particular of stone. Metals came later. The existence of the Bronze Age, however, was not recognized by Kraft.

5. Among some peoples, agriculture must be older than stock-breeding.

6. The later distinction between semiagriculture and full agriculture is foreshadowed.

7. Aboriginal man regarded nature as animated and occupied by spirits. This animistic belief is seen as the result of man's experience during sleep and dreams (*cf.* Tylor and others).

Kraft's view of culture was definitely evolutionary and very similar

to the ideas of the great evolutionists of the nineteenth century. Thus, he claimed that man's intelligence had to develop everywhere in almost the same way.

Kraft's book was soon translated into German and later also into Dutch, but it never became known in wider circles. Ethnology was not yet a science in its own right, and, in fact, cultural evolution was not Kraft's main scientific interest. He had a masters degree, studied physics and philosophy in France in 1742 and was appointed professor of philosophy and mathematics at the Academy of Sor in 1747. He wrote on philosophy and mechanics, and he probably considered this more important than his ethnological digression (Folk, 1960, 2:5–12).

At the beginning of the nineteenth century a larger ethnological work comprising four volumes was published in Danish. The author was a theologian, Bishop C. Bastholm (1740–1819), and the book had the Danish title of *Historiske Efterretninger til Kundskab om mennesket i dets vilde og raa Tilstand* (i.e., *Historical information to the knowledge about man in his savage and rude state*). The four volumes were published in Copenhagen in 1803–1804, but they must be considered very inferior scientifically to Kraft's little book of earlier date. Bastholm does not show any approach to genius and is highly influenced by the naive environmentalism so typical of his day.

Kraft remarked that implements of wood and stone predated those of metal, but he did not divide early culture into the phases of Stone, Bronze and Iron. This discovery was made by Christian Jurgensen Thomsen (1788–1865), who thus became the ancestor of European archaeology. Thomsen was not an ethnologist or an ethnographer in the proper sense of these words, but he had a keen interest in primitive artifacts. At his initiative the ethnographical museum in Copenhagen was founded in 1841 as the first general museum of its kind. However, the idea of ethnographical museums had been advocated before Thomsen by the French geographer E. F. Jomard, who tried to establish such a museum in France but failed. The ethnographical museum in Copenhagen was based upon the very old collection of Frederik III and Ole Worm of the seventeenth century.

Thomsen was an outstanding leader of the museum he had himself created. His successor was the famous archaeologist J. J. A. Worsaae (1821–1885), who took less interest in the ethnographical collections, which had been greatly augmented by Thomsen's indefatigable efforts. The establishment of the ethnographical museum in Copenhagen took

place at a time when Denmark still maintained a small empire with colonies in Asia, Africa and the West Indies, as well as in Greenland. These possessions, of course, stimulated the growth of the ethnographical museum.

During the eighteenth and nineteenth centuries there were also some colonial servants, officers and clergymen who wrote interesting ethnographical accounts. However, ethnology as a science had not yet been properly established in Denmark, and very few of these authors took up theoretical problems. The most important exception to this general rule was Hinrich Johannes Rink (1819–1893). He came to Greenland as a geologist, but he later held very high official positions as inspector of South Greenland and director of the Royal Greenland Board of Trade. Rink became very interested not only in traditional Eskimo culture but also in the problems of acculturation arising from the clash between two very different types of civilization. In 1862 he published a pamphlet with the Danish title *Om Aarsagen til Gronlaendernes og lignende, af Jagt levende, Nationers materielle Tilbagegang ved Beroring med Europaeerne,* (i.e., *The reason why Greenlanders and similar peoples living by hunting, decline materially through contact with the Europeans*). In this paper Rink describes in a functional way the problems of acculturation, and he cites cultural or social distinctions of the kind later put into the categories of mechanical and organic societies by Durkheim (who was four years old when Rink's pamphlet appeared!). Rink's paper was available only in Danish until 1972, when an English edition was prepared by George Nellemann (*cf.* Nellemann, 1966–1967).

It took a long time for ethnology to become an independent discipline at the University of Copenhagen. However, there were university chairs in subjects related to ethnology, and some university professors wrote on pure ethnological topics. One of these scholars should be mentioned, namely, the professor of philosophy N. C. Starcke (1858–1926), who seems to have been the first to criticize fruitfully Morgan's application of the survival theory to classificatory kinship systems. Starcke's work on this classical theoretical problem was first published in German (*Die primitive Familie,* Leipzig, 1888), but an English edition appeared one year later (Starcke, 1889). Starcke had very few followers, but his conception that in general a kinship nomenclature is 'the faithful reflection of the juridical relations which arise between the nearest kinsfolk in each tribe' is quoted with approval by A. R. Radcliffe-Brown (1942, p. 59).

In Scandinavia there is a strong tradition of having university chairs in the history of religion, which also includes the religious life of primitive peoples. One of the professors who held this chair at the University of Copenhagen deserves special mention, namely, Vilhelm Gronbech. He conceived of culture and religion as a connected whole, the details of which could be understood only by an analysis. Gronbech had many followers in Denmark, and it was significant that he underlined the importance of primitive religion for the understanding of religion in general.

Most scholars who have been trained in the history of religion have had little interest in ethnology as a whole. Some of them, of course, also embraced the study of social organization connected with ritual, but few of them have shown an interest in technology and economic organization. Therefore they have had very little contact with the ethnographical museum. At the University of Copenhagen, the study of the material culture and economic organization of exotic peoples has for many years been placed under human geography. This subject has had two distinguished representatives in ethnology. One is H. P. Steensby (1875–1920), who was occupied mainly with the study of environmental factors in the shaping of culture. He used this method to explain historical development and chose for his purpose regions where peoples were living under extremely difficult environmental conditions (the Arctic, the Sahara). Steensby's notion about the origin of Eskimo culture in the Barren Grounds of Canada has been refuted in the light of the archaeological research carried out by American, Canadian and Danish scholars. However, his theory of the origin of irrigation culture in dry mountainous zones still seems to be important. He reached this conclusion on the basis of personal field experience in Algeria and Tunesia (Steensby 1908–1909).

Steensby used the word *'antropogeografi'* for his environmental studies, and he was clearly influenced by Ratzel and the French geographers. This also holds to some extent for the other prominent professor of human geography, Gudmund Hatt (1884–1960). Hatt, however, had also studied anthropology in the United States (with Roland B. Dixon). His interests were extremely wide and far-reaching, as were his independently framed theories on cultural history and development. Unfortunately, Hatt's first important ethnological study (1914) has never been translated into English or any other international language. Its subject was circumpolar skin garments, but the results have been ex-

tremely valuable for the general study of dress. Before Hatt, many geographers and ethnologists (e.g., the German scholar Georg Gerland) believed that the problem of dress development could be solved mainly by reference to environmental factors. Hatt showed that this is an exaggerated view, as culture-historical events must be considered in relation to one another and are not in a one-to-one correlation with natural circumstances. Though a geographer, Hatt was occupied more with the diffusion of cultural traits than with mere environmentalism. He was also an evolutionist in the sense that he understood the evolution of garments from the analysis of their styles. Several articles by Hatt – mainly on culture-historical problems in the Arctic – were published in English. He was fascinated by reindeer nomadism, but though he made a field trip to Lapland he was not a field worker in the modern sense. His main interest was the origin of reindeer breeding, which he saw as originating in a hunting culture, using the first tame reindeer for decoy. Hatt's elaboration of this theory became important to Wilhelm Schmidt and other scholars of the Vienna school. They, however, went much further than Hatt, constructing theories which he did not consider to be confirmed by his material. In his later years Hatt was deeply absorbed in comparative studies in folklore in an effort to throw light upon the question of trans-Pacific cultural connections. But he made himself famous in Danish archaeology by comprehensive excavations of house ruins and agricultural remains from the Iron Age. His major preoccupation was the study of technological and economic development from the cultural level of hunters and gatherers to the industrial revolution; his classification of the various forms of economic production was based upon the division of labor with regard to sex.

Hatt was an eminent scholar, and he strongly influenced Danish students in the various branches of anthropology: Human geography, ethnology, European folklife, folklore and archaeology. Nevertheless, Hatt rarely considered culture in all of its aspects, that is, Gestalt or culture-whole. This had to wait for Kaj Birket-Smith (b. 1893). Birket-Smith was trained as a human geographer; ethnology did not yet exist as a independent subject at the University of Copenhagen. However, his interests were from the beginning ethnological – focused as they were on the American Indians and the Eskimos. He did several tours of field work in Greenland, Canada and Alaska. He always emphasized the great importance of ethnographical field work to ethnological theory, although he considered himself mainly a culture-historian. As such, he

continued his Artic studies along the lines laid down by Steensby and Hatt but was also much influenced by Erland Nordenskiold. Later in his career Birket-Smith developed a profound interest in the cultures of Asia and Oceania, and he had the opportunity to carry out field work in the Phillipines and on Rennell Island.

During most of his working life Birket-Smith has been attached to the Department of Ethnology of the Danish National Museum in Copenhagen–from 1946 to 1963 as the head of the institution. In 1945 he also became the director of ethnological studies, which, at his initiative, became an independent subject at the University of Copenhagen. He established a M. Sc. degree program in ethnology within the faculty of natural science. This degree required geography and archaeology as subsidiary subjects and extensive knowledge of regional ethnology and of material culture. While Birket–Smith has always viewed culture history as the foundation of ethnological science, it is also true that he acknowledged other ethnological schools; students had to read Malinowski, Radcliffe-Brown, Evans-Pritchard, Firth and other nonhistorical disciplinarians. When Birket-Smith retired in 1963, his position as head of the Department of Ethnography was taken over by Helge Larsen. In 1964, however, the university post was changed to an ordinary professorship, which I have held since that time. The following year the University of Copenhagen founded its own institute ('Institut for Ethnologicog Antropologi'), which was placed outside the National Museum in a separate building belonging to the university. Museum activity and university teaching now became separate tasks. The ethnographical collections are still used for technological and regional courses, but they play a minor role in teaching as a whole. The curriculum has been strongly influenced by British social anthropology, but the faculty now looks more to American anthropology as the model. The requirements for the master's degree have also been altered. The student must have an elementary knowledge of archaeology, physical anthropology and linguistics, in addition to one subsidiary subject of his own choice, adapted to the subject of his thesis. The area knowledge requirement has been reduced, and the requirement of theoretical knowledge has been greatly augmented.

Ethnology, as indicated, is grouped within the faculty of natural sciences at the University of Copenhagen because of its original connection with geography. Within the faculty of art an entirely new subject has recently been established: 'cultural sociology' *(kultursociologi)*,

which comprises social science in general, including aspects of social anthropology.

It should be mentioned that there are other chairs of anthropological subjects at the University of Copenhagen. A professorship of European folk culture was established a few years ago (Axel Steensberg), and for many years there has also existed a chair of Eskimo language and culture (Professor Erik Holtved). Chairs of Nordic and classical archaeology are long-standing, as are those of linguistics. Physical anthropology, on the other hand, has never had many followers. A laboratory in physical anthropology is attached to the study of anatomy within the faculty of medicine, and the 'Institut for Etnologicog Antropologi' now has a lectureship in physical anthropology.

In addition to the University of Copenhagen, founded in 1479, there are two other universities in Denmark. The University of Odense was established in 1966, and the University of Aarhus was founded in 1928. The latter university has been organized in much the same way as the University of Copenhagen. There are chairs of several anthropological disciplines, including archaeology and linguistics.

There is also an archaeological museum containing an ethnographical collection. It is directed by Klaus Ferdinand, who is also the head of ethnological studies placed under the faculty of art. The requirements for degrees differ from those in Copenhagen, but the scope and aim of the subject are the same. In Aarhus, as in Copenhagen, ethnology is now organized as cultural and social anthropology, and a major focus is the analysis of contemporary societies. A recent trend in both Aarhus and Copenhagen should be emphasized here: General ethnology is no longer considered a subject dealing exclusively with primitive cultures. Europe, including Denmark, is now recognized as a legitimate field for research.

Anthropology in Norway, Sweden and Finland is only outlined here, but not because I consider Danish anthropology more important and of higher standard than anthropology in the other Scandinavian countries. The reason is simply that as a Dane I know more about the history and modern trends of Danish anthropology.

NORWEGIAN ANTHROPOLOGY

The ethnographical museum in Oslo is almost as old as that of Copenhagen, having been founded in 1857, and it has always been part of the University of Oslo. This is one reason why chairs of anthropology were established far earlier in Oslo than in Copenhagen. In 1890 the director of the museum, Yngvar Nielsen (1843–1916), was appointed professor of 'geography and ethnography'; in 1903 the title of his professorship was altered to 'ethnography' only. However, Nielsen was neither an ethnologist nor an ethnographer, and geography remained his main interest. He was succeeded by Ole Solberg (1879–1946), who was both an ethnologist and an ethnographer. Solberg undertook ethnographical field studies among the Swedish Lapps and the Hopi of the American Southwest, but his particular field of interest seems to have been circumpolar archaeology. He was, until 1940, strongly influenced by German culture and ideas, but most of his students did not become culture historians in the narrow sense of the word. It should be noted that by the 1930s the methodology of culture history was under severe attack by Albert Brock-Utne, who was trained in theology and the history of religion but had had also studied with Malinowski in London.

Gutorm Gjessing followed Solberg as head of the ethnographical museum and professor of ethnology at the University of Oslo. He was trained in Nordic archaeology and is known for his impressive corpus of innovating archaeological studies. However, Gjessing turned to ethnology and did not remain a culture historian. Several visits to American and British universities later influenced him in the direction of the modern social sciences and social philosophy. It remains true of the ethnologists at Oslo University that their studies are oriented toward contemporary societies and that they largely neglect culture history. Recently, an ethnological institute has been established at Oslo University, in addition to the ethnographical museum. However, there is little difference, if any, between the emphasis and methods of scholars at the museum and those at the institute.

The chair of social anthropology founded after the war at the University of Bergen has played a critical role in the development of ethnological theory in Norway. Thus far, this is the only 'social anthropology' chair at a Scandinavian university, and it has no connection with the ethnographical museum which forms part of the historical museum at the University of Bergen. The professor of social anthro-

pology in Bergen is the well-known scholar Frederik Barth. Trained in Chicago, he later became attached to British universities. Barth has been most constructive as a methodologist, and his work has strongly influenced the younger generation of ethnologists, not only in Norway but also in Sweden and Denmark.

Norway was the first Scandinavian country to adopt the modern methodologies of functionalism and structuralism at the expense of culture history. At the Universities of Bergen and Oslo it is considered ideal that Barth's master's thesis was written on the basis of personal field work. In consequence many students select Norwegian farming or fishing communities as a subject for field work. Therefore, general ethnology i.e., social anthropology, has approached Nordic ethnology (folklife research) in popularity and importance. The representative of this topic at the University of Oslo, Professor Knut Kolsrud, was trained in general ethnology at the Universities of Oslo and Chicago.

Swedish anthropology

One of the first general ethnologists in Sweden was Hjalmar Stolpe (1841–1905), who turned from zoology to archaeology and then to ethnology. He became director of the ethnographical museum in Stockholm in 1900 and was appointed professor of ethnology *(etnografi)* in 1903. Stolpe was an evolutionist who concentrated on the evolutionary problem of primitive art. His main interests were the aboriginal cultures of America and the Pacific.

The later head of the museum and professor of ethnology, Gerhard Lindblom (b. 1887), is known for his excellent monograph on the Akamba of East Africa. The balance of Lindblom's work deals with comparative studies of African culture traits. The perspective and method are those of the diffusionists, though not adapted to the school of German and Austrian *Kulturkreis* scholars.

The professor of general ethnology at the University of Uppsala, Sture Lagercrantz, became Lindblom's first student in Stockholm. Like Lindblom, his main field of interest is Africa. He is also a diffusionist and as such is strongly influenced by German scholars–in particular, Herman Baumann.

As head of the museum and professor in Stockholm, Lindblom was followed by S. Linne, a specialist in the ancient American civilizations

who subsequently developed a strong archaeological interest. He has now formally retired. This professorship has until recently been named *'professor i etnografi'* but is going to be changed to *'professor i etnografi, sarskilt sosialantropologi'* (i.e., 'ethnology, with particular reference to social anthropology'). At the same time, the posts as museum director and professor are going to be divided. Teaching is still done in the ethnographical museum, but a new building is being planned for the university institute.

In Gothenburg, ethnological science began with the famous scholar Erland Nordenskiold (1877–1932), who from 1924 served as both director of the ethnographical museum and university professor. He made several expeditions to South America, but his ethnographical studies were noted for his deep interest in culture history and the diffusion of culture traits. This was ever more true of his successor at the museum, Walter Kaudern (1881–1942). But a change in perspective at Gothenburg was introduced by K. G. Izikowitz, professor in general ethnology at the University of Gothenburg. Until 1967 he was also head of the ethnographical museum; the two positions have now been separated. Izikowitz studied under Nordenskiold and was trained in culture history and diffusionism. However, he soon became interested in structural functionalism. He has studied in England, France and the United States and has conducted field work in Indo-China and in India. And he has dissociated himself from the culture historical methods of German and Austrian scholars.

As in other Scandinavian countries, the history of religion has played an important role at Swedish universities. Several scholars in this area have developed true ethnological interests; the present professor of the subject at the University of Stockholm, Åke Hultkrantz, has a particular interest in the religions of American Indians and is deeply engaged in problems of anthropological method.

Nordic ethnology or folklife research has been of special importance in Sweden and, under the leadership of Sigurd Erixon, a cultural historian though influenced by sociological views, has gained prominence throughout Europe. His successor at the University of Stockholm, John Granlund, has, in his later years, shown a strong interest in social anthropology of the British and Norwegian type, although other Swedish folklife researchers have dissociated themselves more or less from the culture historical method, e.g., Albert Eskerod, who early became a functionalist.

Finnish anthropology

In Finland the European and non-European branches of ethnology are less distinguishable from each other than in other Scandinavian countries. There also seems to be a tendency toward merging ethnology with sociology. Finland's internationally renowned scholar Edvard Westermarck (1862–1939) trained such versatile students as Rafael Karsten, Ragnar Numelin and Gunnar Landtman. Nevertheless, outside of Finland he does not seem to have been very influential on Scandinavian ethnology.

Summary

The trends of Scandinavian anthropology (ethnology) can be summarized as follows:

1. At least three Scandinavian scholars (Kraft, Rink and Starcke) had constructive ideas which were far ahead of their time. There are several reasons for their failure to influence anthropological science internationally: The scientific milieu was small, they wrote too little and their writing was mostly in Danish.
2. The Dane Christian Jurgensen Thomsen became the founder of the first ethnographical museum. Theoretically he proved most influential within archaeology through his division of ancient cultures into Stone, Bronze and Iron Ages. A national interest in Archaeology arose early in all Scandinavian countries. This, no doubt, has influenced ethnology by directing it toward the study of material culture and museum activity. There was also a close relationship between ethnology and geography. At the University of Copenhagen ethnological study was even considered a part of geography until the Second World War. Before 1939 many Scandinavian scholars worked on problems of material culture and economic organization, and most of them were culture historians.
3. In all Scandinavian countries there have been a few scholars with particular interests in 'spiritual' culture and social problems. Often, these scholars were neither anthropologists nor ethnologists in the proper sense of the terms; some were associated with such subjects as theology, history of religion, philosophy, etc. They were not interested in material culture or in culture history as such.

4. Since World War II diffusionism has declined, especially in Scandinavia. This is true, in particular, for Norway, but Sweden and Denmark are following the same trend. The diffusionist tradition has been strong at the University of Uppsala, but even here other perspectives are gradually becoming more influential. Some years ago it might have been said that functional and structural schools were predominant in Norway, culture history and/or diffusionism in Sweden (apart from Gothenburg), while Denmark tried to maintain an intermediate position. This is hardly the case today. In all countries there is a tendency toward functional and structural studies of contemporary societies. There is also a tendency for students of general ethnology to undertake studies of Scandinavian communities. Simultaneously, the university chairs have been moved out of the ethnographical museums, and new institutes of ethnology or social anthropology have been established.

A PERSONAL CONCLUSION

I agree with Hultkrantz (1965, p. 18) that we need more interdisciplinary collaboration in Scandinavia. I therefore appreciate the initiative taken in this direction by 'The Nordic Council for Anthropological Research'. However, as conceived by the younger generation of Scandinavian anthropologists, ethnology, or social anthropology tends more toward sociology than toward the traditional anthropological disciplines. Anthropology (ethnology) and sociology must, of course, collaborate. I disagree, however, with those scholars who seem to claim that anthropology should be considered a sort of sociology. If anthropology (ethnology) is to persist as a science in its own right, it must be able both to collaborate with sociology on a wide range of topics related to the study of man and to make use of more than one method. Here, I do not exclude culture history or diffusionism.

To clarify this view, and its origins, I may perhaps be allowed to give you its background:

I started my ethnological studies under Birket-Smith and was trained to make use of the cultural historical method. I was, and still am, interested in culture history, and on the whole I do not believe that its method is logically wrong. The historical dimension is necessary to anthropology, whether one calls it social or cultural. I also appreciate Birket-Smith's requirement that students have a knowledge of eth-

nography. I have often felt that ethnography enables me to evaluate the soundness of anthropological generalizations.

In the early 1950s I had the opportunity to study for more than two years at London University. This was due the kind initiative taken by Professor Daryll Forde, toward whom, as toward my supervisor at the University College, Dr. Phyllis Kaberry, I am greatly indebted. Though I had already read the writings of several British anthropologists, the training I received in London was entirely different from what I was used to. In time, I became acculturated to the value of the British concept.

As it happened, I became interested in pastoral nomadism and undertook my field work among the Arab bedouins of Algeria on two short trips. Later, I shifted to the Tuareg of the desert and have completed three years of field work among them. As I penetrated more and more deeply into the riddles of Tuareg culture, I became convinced that I should have to use more than one method in order to make a comprehensive scientific analysis. It is obvious that the cultural historical method is useful, and one realizes this more and more as knowledge of North African cultures increases; I also believe it possible to frame historical theories more precisely within limited areas. But there can be little doubt of the usefulness of structural-functional method for the analysis of Tuareg culture. This, for instance, holds for the study of certain types of kinship behavior, including joking and avoidance. The salient features of Radcliffe-Brown's (1942, pp. 90–116) theory seem to be confirmed also by the Tuareg – so perhaps folk-models are not always poor models. But if one tries to understand Tuareg kinship terminology and their kinship behavior, one finds that the Tuareg often neglect the functional and structural principles laid down by Malinowski, Radcliffe-Brown and Lévi-Strauss, to mention the three most prominent scholars of nonhistorical schools. As I see it, one must go back to history to fully understand the kinship system of the modern Tuareg, using Arabic sources from the Middle Ages, along with native traditions and accounts, and applying diffusionist principles make it possible to explain certain peculiarities of Tuareg social organization.

To conclude, I have been trained in the method of two distinct anthropological schools and have found both of them useful in the sense that they throw light on the critical problem of scientific analysis. In this sense, the traditions of modern anthropology can be seen as complementary rather than antagonistic.

References

Birket-Smith, Kaj (no date), *Jens Kraft: A pioneer of ethnology in Denmark.*

Hatt, Gudmund (1914), *Arktiste Skinddragter i Eurasien og Amerika.* Copenhagen.

Hultzkrantz, Ake (1965), Anthropology as a goal of research: Some reflections, *Folk,* 7: 5–22. Copenhagen.

Nellemann, George (1966–1967), Applied anthropology in Greenland in the 1960's: H. J. Rink's administration and view of culture, *Folk,* 8–9: 221–241. Copenhagen.

Radcliffe-Brown, A. R. (1942), *Structure and function in primitive society.* London.

Starcke, C. N. (1889), *The primitive family.* London, The International Scientific Series, vol. 66.

Steenby, H. P. (1908–1909), Nogle etnografiske Iagttagelser fra en Rejse i Algier og Tunis 1908, *Geografisk Tidsskrift,* 19: 269–185; 20: 24–39, 65–76, 185–192. Copenhagen.

12 India: A Western apprentice

SURAJIT SINHA

1. The problem

The process of the naturalization of the different strands of Western anthropological traditions is discussed in this paper, and an attempt is made to find out whether a distinctively Indian approach to the subject has emerged. It is assumed that the following are some of the important factors that have influenced the development of anthropology in India: (1) The time at which the various aspects of the subject were introduced from the West; (2) the unique sociocultural milieu and phase of politicoeconomic development within which Indian anthropologists operate; and (3) the ideas of her outstanding social and political thinkers.

2. The historic phases

According to the earlier papers of Bose (1952, 1954, 1963), Majumdar (1956) and Vidyarthi (1966), the career of Indian anthropology may be divided into the following distinct historic phases: Formative (1774–1919), constructive (1920–1949) and recent (since 1950).

2.1 The formative phase (1774–1919)

This phase was initiated mainly by British administrator-scholars and European missionaries and was marked by the following features: A natural history approach, description of diversity of customs and emphasis on the tribes. The various encyclopaedias of castes and tribes

and the early tribal monographs appeared during this period. The prevailing intellectual interests of these pioneers were the search for primitive survivals from the classical evolutionary point of view and the attempt to understand the culture history of the people of India by studying the distribution of customs, languages and physical forms. These pioneers were, in general, committed to a conservative social ideology of preserving the multiplicity of customs. As a result, unique rituals are emphasized in these early accounts, and rigorous description of economic organization and trends of change are neglected. Indian pioneers such as Aiyar and Roy were guided by essentially the same approach in looking into the Indian scene. It should be mentioned here that whereas ethnographic reports of this period were mainly formal listings of customs, many of the administrative reports, particularly land revenue settlement reports, provided a realistic functional picture of Indian rural society. The works of Baden Powell in particular come to mind. But these latter perspectives were not integrated with the prevailing anthropological tradition of this period.

2.2 The constructive phase (1920–1949)

During this period a few Indian universities started teaching anthropology, and about half a dozen Indian anthropologists received their training abroad. The predominant influence continued to be from Britain, the main exponents being Rivers and Malinowski. Study of kinship attracted considerable attention, and a few tribal monographs, with functional orientation, were published toward the end of this period. There was also some impact of the American diffusionist school and the later developments in acculturation studies. Although some of the missionary scholars represented the German-Austrian diffusionists, the impact of this school on Indian anthropology has not been very significant. Some Indian anthropologists were already becoming interested in the study of applied problems, namely, conditions of industrial laborers, the future of the aborigines, the impact of famine on Bengal society and so on.

2.3 The recent phase (since 1950)

This period has been marked by the rapid predominance of American anthropology in the Indian scene and substantial governmental patronage of anthropological teaching and research. The influence of modern British anthropology, however, continues to be important. These in-

fluences and the availability of resources have stimulated the growth of a professional cadre of anthropologists in India. A Ph. D. degree in anthropology, foreign or Indian, is becoming the minimum qualification of a professional anthropologist.

In recent years the attention of the Indian anthropologists has shifted substantially from the tribes to the peasants, and interest has also developed in the study of the more complex levels of Indian culture, namely, the urban and industrial centers. The foreign anthropologists who have had considerable impact on the Indian scholars of this period are: Robert Redfield and his collaborators in the U.S.A., British structural anthropologists following Radcliffe-Brown and the followers of Lévi-Strauss, Dumont and Leach. There has been a phenomenal increase in the number of books and articles published by Indian anthropologists, a few of which have appeared in important Western journals of anthropology. The publications and concepts of a few Indian anthropologists have been widely referred to by Western scholars on India.

In general, Indian anthropologists have been prompt in responding to the latest developments in the West. In so doing they have declined to pursue the earlier phases of constructive endeavors to their logical ends in the Indian context.

3. Involvement in problems of direct culture change

Apart from routine socioeconomic surveys involving schedules and questionnaires, Indian anthropologists have also been concerned with several general problems of culture change. One area of concern has been the determination of whether the caste system is disintegrating or whether it has been strengthened since independence. Similarly, study has been made of the impact of adult franchise, urbanization and industrialization on the caste system. A second major field of study has been the nature of Indian unity and the characterists of various categories of subnationalism. Third, the role of Indian social and religious traditions in economic development have been considered. (Here, the ideas of Max Weber have received considerable attention.)

4. Interest in the study of the total pattern of Indian civilization

Scholars of the early and formative periods of Indian anthropology were already interested in the study of Indian civilization as a culture-historical problem embracing a variety of races and cultures and of evolution from a primitive base. Apart from these early attempts at reconstruction of culture-history, a few Indian anthropologists have also tried to systematically conceptuatize the total structure of Indian civilization. A brief outline of some of the major scholars and their approaches follows.

Bose – India as a composite of two basic zones of material culture cutting across the linguistic divisions; pyramidal form of Indian unity in diversity; noncompetitive economic ideology and the Hindu mode of tribal absorption; interrelationship between the village and the supralocal centers.

Srinivas – Sanskritization, the role of the dominant caste in local and regional integration.

Karve – Agglomerative character of Hindu society.

Sinha – Indian society as 'evolutionary emergent' from a tribal base.

Vidyarthi – Study of sacred complexes.

It would be interesting to determine whether the fact that most of the outstanding professional anthropologists come from the upper strata of Hindu society has influenced their conceptualization of Indian civilization.

5. Contributions in research methodology

It is surprising that Indian anthropologists, in general, have not given much systematic attention to devising special research methodologies for studying the unique layout of the cultural patterns and processes of the Indian subcontinent. Only a few exceptions strike our attention in this regard: Das (rigorous utilization of the genealogical method in the study of Parum social organization); Bose (application of spatial distribution techniques in the dating of Indian temples, utilization of the tools of human geography in studying culture-historical problems and use of family histories in the study of social change in urban centers); Chattopadhyay and Mukherjee (use of statistical techniques in studying social change); Karve (collation of textual analysis with field data in

the study of kinship); and Vidyarthi (study of sacred complex by utilizing such concepts as sacred center, cluster and segment).

6. Involvement in political ideologies

In a developing society like India, where political systems have not yet attained a stable equilibrium, it would normally be expected that a number of anthropologists, as commentators on contemporary Indian society, would be ideologically committed. This, however, is not the case. With the expection of a few who profess preference for Gandhism or for Marxism, the majority of Indian anthropologists are politically uncommitted in their writings.

7. Distinctive institutional set-up for teaching and research

Up to 1944, anthropology was taught as a full-fledged department in only one university. Now there are about a dozen university departments of anthropology and several sociology departments in which social anthropology is integrated into the curriculum. There is a controversy among Indian anthropologists as to whether social anthropology should be combined with sociology or whether anthropology should stand apart as a distinct, integrated discipline. By the same logic, there is debate about whether the departments of anthropology should belong to the natural science or the social science divisions.

A distinctive feature of recruitment in the universities in postindependence India is that teachers and research scholars are increasingly being selected from the local language groups. As a result, most of the universities are not operating on an all-India basis of recruitment. This trend toward regionalism and parochialization in the universities and also in the tribal research institutes of the various states has been partly mitigated by the establishment of a number of government–sponsored–directly controlled and semiautonomous–all-India institutes, the most important of which, for the anthropologists, is the Anthropological Survey of India. Some of the newer institutes promote an interdisciplinary approach utilizing the latest quantitative methods developed in the West. The National Institute of Community Development, the Indian Institutes of Management and the UNESCO Research Center for the

Developing Nations in South and Southeast Asia belong to this category.

It appears that this institutional situation is creating two types of professionals among Indian anthropologists. Those in the universities, except for the Central Universities, are under pressure to be inward- or regional-looking, whereas the better financed, semiautonomous, interdisciplinary institutes are predominantly outward-looking. Their research interests tend to be more directly stimulated by recent Western (particularly American) assumptions.

It thus appears that Indian anthropologists today are being influenced by either a parochial, regional scene or the Western-dominated external world; in either case, communication in an all-India framework is not growing vigorously. Only feeble pan-Indian links for the discipline are provided by the Anthropological Survey of India, contacts between university teachers as external examiners and occasional seminars and annual meetings of the Indian Science Congress Association.

8. Is there then a distinct Indian tradition in social cultural anthropology?

From a review of the foregoing points it will appear that certain distinct features do characterize the Indian pursuit of anthropology.

1. The attention of the Indian scholars has been virtually limited to the Indian scene.

2. Indian anthropologists have seen their society and culture through largely English eyes and have quickly adopted the latest phases of research interest on India among the Westerners. Yet spontaneous, sympathetic immersion in their own civilization has made it possible for a few Indian anthropologists to provide some original and incisive insights into the study of the total structure of Indian civilization. Here, the conceptualization of the civilization may have been influenced to a certain extent by the caste and community background of the Indian anthropologists.

3. The general acceptance of an intellectually dependent role has inhibited most Indian scholars from vigorous and independent exploration in the field of general theory or research methodology in anthropology.

4. Compared to their foreign counterparts, proportionally more Indian

anthropologists have been involved in applied problems. Due to extensive government patronage of the various programs of directed culture change, Indian anthropologists are, in general, conscious that they have a role to play in nation-building. However, here again, most of their concepts and tools have been borrowed from the West.

5. Apart from involvement in specific problems of directed change, a few Indian anthropologists have also been interested in the basic ideology of change. Here, Marxism *vis-à-vis* Gandhism has played some part. The ideas of outstanding literature like that of Tagore have also influenced certain anthropologists.

9. Prospects

Westerners, particularly the British, brought the great gift of the natural history tradition to India. Unfortunately, British rule also resulted in a persistent intellectual dependence among Indian scholars, which has characterized their adoption of the successive phases of development in Western anthropology. Since Independence, a section of Indian leadership has been concerned with achieving 'modernity' at a fast rate. It is not unlikely that such a prevailing ideology has facilitated the transmission of the latest Western European conceptual tools, as well as fashions, in anthropology. One of the results of this has been that Indian anthropologists have generally avoided the basic task of completing a descriptive outline of the culture forms of the Indian people; lacking that, they have been unable to undertake analytical and incisive studies of specific problems based on the ethnographic record. It is only on the basis of such ground work that Indian scholars will be able to participate effectively in the international adventure of expanding the frontiers of anthropological knowledge.

Since the research interests of a nation are circumscribed by its particular phase of history, it is unlikely that Indian anthropology will find a strong domestic orientation in the near future. For some time, the proliferation of trained manpower, random efforts at catching up with the latest developments in the West and a general increase in the number of publications will characterize the development of Indian anthropology.

13 Japan: An Asian reflex

CHIE NAKANE

Anthropology, that is, ethnology or cultural anthropology, is one of the least-developed social sciences in Japan. One reason lies in the geographical and historical situation of the country. For a people of homogeneous culture, isolated from other neighboring peoples not only by the sea but also by national policy for nearly three centuries prior to modernization, it is understandable that they failed to develop a genuine interest in the comparative study of peoples or cultures. Even if such a people are curious about what is going on outside their own world, this kind of interest does not necessarily result in a systematic and objective comparison of different cultures and peoples but rather in the acquisition of imported knowledge which offers stimuli for their own growth. In such a case, a comparison takes the competitive form of 'we' and 'others', not 'A' and 'B' in equal weight without involving the observer's situation.

A comparative, scientific anthropological method as developed in the West is still in its beginnings in Japan, even among those who call themselves ethnologists or social anthropologists. Traditionally, therefore, the Japanese scholar's ultimate interest has been confined to Japan–a comparison applied to local cultural variations in the population of Japan and to different stages in Japanese history. This tendency is still dominant among contemporary anthropologists and in related fields. Therefore, what I present here may not fall in the category of anthropology in a strict sense; my purpose is to explicate the *roots* of Japanese anthropology.

1. TOKUGAWA SCHOLARS

The embryonic form of interest in different customs is found among traditional scholars of the feudal Tokugawa period (1603–1868). During that period the absence of foreign entanglements had stimilated the rapid growth of the national economy, based on the social stability attained under the inward-turning Tokugawa policy, and had greatly facilitated the development of city life. Consequently, contrasts between urban and rural cultures became sharper. This, in turn, stimulated certain scholars (who were city dwellers) to a concern with local customs. There are, therefore, considerable numbers of studies, including travelers' accounts, dealing with customs in different local areas in Japan. One fairly well-organized report is known as *Fūzoku-toijō (Questionnaire of customs and manners)* by Yashiro Kōken. Though he himself did not undertake a field survey, he distributed a large number of questionnaires to people in different localities and collected information about calendric festivals, funerals, marriages, values and peasant material culture.

Such local customs were interpreted by two types of scholars: *Kangakusha* (experts on Chinese studies, or those who applied Chinese research desciplines); and *Kokugakusha* (those who applied a Japanese approach, without depending on Chinese disciplines, and based their work on the study of ancient Japanese history and literature).[1] In dealing with local customs, *Kangakusha* interpreted them according to the patterns of Chinese literature. On the other hand, *Kokugakusha* sought to identify rural customs with those of ancient Japan; that is, they believed that rural customs echoed ancient patterns. Deeper concern with local customs was found among the *Kokugakusha*.

1. *Kangakusha* were certainly dominant in both number and influence; they represented what were then called scholars. They also contributed in the introduction of modern Western ideas in the process of Japan's modernization. They were stronger in the understanding and interpretation of foreign ideas and in logical constructions than were *Kokugakusha*. These two approaches are characteristic of the Japanese scholastic tradition, which is to a certain extent recognizable even today. For example, among modern scholars there are two divergent approaches: one always seeks confirmation in Western theory whereas the other tends to reject Western theory in favor of searching for the Japanese way. The weakness of the latter, including the *Kokugakusha* of the feudal age, is that their way of thinking is hard to comprehend, partly due to their lack of a logical framework.

2. An early stage of acceptance of Western ideas

The great influx of Western ideas on the modernization of Japan led the people to devalue old customs and to eagerly accept new, Western ideas. The people at large generally came to regard folk customs as those of primitive people. Japanese scholars were encouraged to take up Western anthropology, which conventionally dealt with primitive peoples, in order to investigate and interpret rural customs in their own country.

The term 'anthropology' first appeared in Japan in 1884 when several scholars, led by Tsuboi Shōgorō who studied anthropology in London, formed *Jinruigakukai* (The Anthropological Association), which evolved into the establishment of *Tokyo-jinruigakukai* in 1886, with the publication of the journal *Jinruigaku-zasshi*. The journal originally published a variety of articles in physical anthropology, ethnology, linguistics, prehistory, the history of manners and customs and folk beliefs. Ethnological essays were dominant. Earlier issues of the journal reveal the major concerns and perspectives of these scholars. For example, there is an article entitled 'A study of customs related to marriages in Japan' by Watase Shōzaburō, in which Japanese marriage customs are interpreted in a manner employed by Western scholars for primitives. Another article, 'Marriage rules in ancient Japan' by Miyake Yonekichi, presents the view that a pattern found at an early stage of human history is also found in an ancient period in Japan. There are obvious influences of Morgan and Tylor in these articles. In those days Japanese scholars were greatly interested in the evolutionary view in terms of a universal human history rather than in existent primitive peoples outside of Japan.

However, their strong ethnological interests, stimulated by the works of Western scholars, further led them to establish *Dozoku-gakukai* (The Ethnological and Folkloric Association) in 1894. At this stage, Japanese scholars were interested in the various aspects of the anthropological sciences, but they were not aware of differences in the scope and method of ethnology and folklore. Nevertheless, Western methods and approaches began to be applied to Japanese materials. One of the earliest such articles was 'Folk-belief in *Chuou* (a district in northern Japan)' by Anesaki Shoji, which appeared in in *Tetsugaku-zasshi* (Journal of Philosophy). Asesaki was much influenced by Frazer and Tylor. In general, the influence of Frazer dominated anthropological studies in Japan at the beginning of the twentieth century.

Following the 'Frazerian' period, the contribution by Minakata Kumakusu, a well-known biologist, deserves note. Minakata compiled a kind of encyclopedia of customs and manners, twelve volumes in all, based on his far-reaching data from Japanese, Chinese and Western sources, shaped in large part, it is said, by his own research in the British Museum.

While Minakata was devoting himself to the study of customs through written sources, Yanagita Kunio, the father of Japanese folklore, initiated his research based on field work in the various rural areas in Japan, although he was apparently stimulated by Minakata's literary efforts. But it is Yanagita who emerges as the seminal figure in the founding of ethnology in Japan.

3. Toward the establishment of folklore and ethnology in Japan

A gifted scholar, Yanagita Kunio (1875–1962), initiated his research on folk culture in his mid-30s and was productive for about 40 years. He was indeed self-trained, graduating from the Faculty of Law at the University of Tokyo and thereafter entering government service. He spent a couple of years in Europe (mostly in Geneva and London) for the government; there, he collected a number of books which encompassed the leading contributions of Western shcolars in folklore and anthropology. During this period he happened to meet Boas in a bookshop in Zurich, and, according to Yanagita, it was Boas who made him realize the difference between *Folkskunde* and *Volkerkunde*. He also met Frazer in London. However, he did not study under any Western scholar. Yanagita was familiar with anthropological researches in the West; unlike his Japanese contemporaries however, he never adopted a Western theory or view. Indeed, he hardly ever referred to Western works for his interpretation of Japanese materials; even his students were not aware of his profound knowledge of Western anthropology. Nevertheless, the influence of Western ideas in his works is found in his basic approach, which reflects the spirit of modern Western science. Though his manner of presentation is distinctly Japanese, he critized the unscientific approaches of his Japanese contemporaries.

In those days, particularly between the second and fourth decades of this century, Japanese scholars enthusiastically engaged in following up the leading theories of Western scholars. Durkheim, Mauss,

W. Schmidt, Malinowski and Radcliffe-Brown were the most popular. A number of translations were published, and reviews of their works filled Japanese journals. Students learned most of the theories of Western social scientists through books written in Japanese. Japanese scholars were occupied in discussing these Western views, but they rarely either elaborated their own views or carried out field work. They were very much in the mold of the *Kangakusha* of the Tokugawa period. It was in such a scholastic climate that Yanagita undertook his unique studies based on field data. He insisted that research and theory must be based on empirical data, and he criticized Japanese scholars for the general dependence on foreign works and their failure to interpret facts for themselves.

His field experience commenced at the beginning of the twentieth century, in connection with his government work–setting up local agricultural associations in rural areas. His earliest books, based on his field study, were published as *Nochikarikotoba-no-ki* (1909), *Toono-monogatari* (1909) and *Ishigami-mondo* (1910), and they stand as classics in the study of Japanese folklore. In these works, Yanagita expressed his view that the significance of the study of rural customs and dialects was not in their exoticism but in their contribution to an understanding of Japanese culture as a whole. And he held the view that differences in local customs and dialects did not come from isolated cultural elements but could be interpreted within the framework of an integrated Japanese culture. Comparisons of local differences, he believed, would reveal the historical and psychological dynamism which defines Japanese culture. This view, coupled with his warm empathy with the life of peasants, an undercurrent of his works, opened the eyes of many Japanese about rural culture, and he gained the enthusiastic collaboration from country people all over Japan. In 1912 Yanagita organized *Nihon-minzoku-gakukai* (The Association of Japanese Folklore) and in the following year began to publish the journal *Minzoku (Folklore)*.

As his work accelerated and his research became more and more advanced, he attracted many gifted scholars in related fields, whom he influenced and collaborated with: Anesaki Shoji, who paved the way for a study of what he called 'the ethnology of religions', and Shiratori Kurakichi, a distinguished Orientalist who specialized in Central Asian culture history. Others influenced by Yanagita were Origuchi Nobuo, foremost interpretor of Japanese ancient literature, and Kindaichi Kyosuke, a student of Ainu language and literature. Among the younger

generation (who now occupy the oldest age group), there were Ariga Kizaemon, a rural sociologist who established village community studies in Japan; Oka Masao and Ishida Eiichiro, who represent the present generation of Japanese ethnologists (both studied in Vienna under W. Schmidt); and Hori Ichiro, well known for his contribution to the study of folk religions in Japan. These are only a few examples of the numerous scholars in folklore and related fields who had close contacts with Yanagita.

Owing to Yanagita, the study of Japanese folk culture had taken its shape under the name of Japanese folklore. However, interest in ethnology–the real interest in peoples and cultures outside Japan–was still underdeveloped among Japanese scholars. Although Yanagita intentionally concentrated on his field work in Japan, younger scholars began to realize that their interests need not be confined to their own country. These scholars had been students or had had close contact with Yanagita and his peers, as mentioned above. Gradually they began to form a group, and in 1934 they established *Minzokugakukai* (Ethnological Society). At the same time Uno Enku, a student of Anesaki, carried out a field survey in Southeast Asia (mostly Java and Sumatra) and published an ethnography of folk rituals related to paddy agriculture in that area (*Malaisia niokeru tōmai-girei*, 1941). On the other hand, under the influence of Shiratori, a cadre of Orientalists specializing in the history of neighboring areas of China, became interested in ethnology,[2] and some of them became active members of the Ethnological Society.

In the late 1930s, when the Second World War was approaching, *Minzoku-kenkyujo* (The Institute of Ethnology) under the Education Ministry was established by members of the Ethnological Society, including scholars who were interested in anthropology. Following the Japanese military invasion of the Continent, the Institute sent scholars

2. These scholars developed their interest in ethnology while they were dealing with historical accounts in Chinese records on various peoples in Asia. The Chinese, unlike the Japanese, have had a considerable ethnological interest in peoples of Chinese periphery. In Chinese literature, there are a number of excellent reports about peoples in Asia, dating from the beginning of the Christian era. One of the best examples of these accounts is the record of Yuantsan, which includes descriptions of the various countries and peoples in Central Asia he encountered on his way to India to study of Buddhism in the period A. D. 629–645. Yuantsan reveals himself as a distinguished anthropologist and geographer and a dedicated scholar of Buddhism.

to north China, Manchuria and Mongolia for their field surveys. Unfortunately, these surveys were shortlived, as the war turned out badly for the Japanese.

During the period prior to and during the war, some anthropological field work was also done in Micronesia by Sugiura Kenichi, and aborigines in Formosa were subject of studies by Mabuchi Toichi and others. Despite the development of Japanese ethnology, these researches were not satisfactory. Due to political instability, lack of funds and inferior academic training, none of these undertakings was comparable in standard to the representative works of British and American anthropologists of the same period. On the other hand, there were important contributions in archaeology on the Continent during the same period. Anthropology was, in fact, more retarded than archaeology in Japan. Moreover, ethnologists suffered from the lack of anthropological interest on the part of the Japanese people at large. It was only during the war that the Japanese showed some interest in peoples of surrounding areas, became familiar with Western anthropological works and subsequently published translations of many monographs on various peoples in Asia and the Pacific.

4. Contemporary situation of anthropology in Japan

Soon after the war, Japanese ethnologists found themselves far behind their contemporaries in Britain, the United States and elsewhere. Since then, American anthropology has been imported under the name of 'cultural anthropology', a novelty for Japanese scholars. Ruth Benedict's *The chrysanthemum and the sword* became very popular, among other works, and American psychological approaches gained many adherents. Japanese intellectuals who had shown no interest in 'ethnology' were now attracted to 'cultural anthropology', and this was combined with their appreciation of those aspects of American culture that were being introduced into Japan in the aftermath of war. Supported by such a postwar climate, the enthusiastic efforts of the prewar ethnologists finally led to the establishment of the Department of Social Anthropology at Tokyo Metropolitan University and the Department of Cultural Anthropology at the University of Tokyo. In addition, chairs of cultural anthropology began to be set up in various other universities.

Following the popularity of American cultural anthropology, par-

ticularly its psychological variant, British social anthropology was the next to attract the younger generation. More recently, the works of Lévi-Strauss, under the term 'structuralism' became fashionable among Japanese intellectuals as well as among anthropologists. Whether these new approaches have really become rooted in Japanese anthropology remains doubtful. In spite of the frequent allusions to these 'new' theories in scholarly discussions, the majority of works by Japanese anthropologists bear on primarily ethnological concerns rather than structural analysis. The influence of prewar ethnology is still strongly felt, and that basic approach is shared by many contemporary anthropologists. Such a tendency originates in the general intellectual climate, still strongly influenced by the German theoretical assumptions which dominated prewar humanities and social sciences in Japan. In the social sciences, Marx and Max Weber are still paradigmatic figures. Social scientific analyses, particularly as developed by English-speaking peoples, have not yet been fully accepted by Japanese anthropologists; that will take more time.

This fact also related to the attitude toward, and meaning of, field work subscribed to by Japanese anthropologists. Field surveys became very popular after the war, and a number of scholars have carried out what they call 'field work' in Japan and abroad. But most of this field work has been of short duration (lasting from one week to three months), and very few researchers have stayed in the field for more than one year.[3] Along with inadequate time in the field, many anthropologists lack proper training. Therefore, in spite of a great quantity of anthropological reports (99 percent of then written in Japanese), their quality is not, on the whole, satisfactory.

This kind of field work, coupled with the lack of a tradition of anthropological field work, is a decided handicap in working overseas. As a matter of fact, if the works done abroad and in Japan are compared, the contribution of the latter is much greater. For example, representative works by Japanese anthropologists are most evident in a series about the origin of the Japanese people[4] and in many studies of rural

3. For the field work done by Japanese anthropologists, see 'Tokushu: Jittai-chōsa (Classified list of field work by members of the Japanese Society of Ethnology–in ethnology, sociology, linguistics, archaeology, physical anthropology and related sciences), *Minzokugaku-kenkyu* 28 (1), 1964.

4. One of the landmarks of this issue is the discussion presented in *Nihon-minzoku no kigen (The origins of the Japanese people)*, edited E. Ishida *et al.*, 1958.

Japan. However, when we examine the work of anthropologists on Japan, we discover that they form only a fraction of the total contributions; most of the contributions are studies focused on rural Japan by scholars in various other social sciences.[5] When anthropologists' work on Japan is compared with that of other social scientists, the former do not necessarily reveal the strength of anthropology. Since Japanese anthropologists are not for the most part, acquainted with a field outside Japan, their weakness lies in the lack of a comparative method, the very basis of anthropology.

If anthropology is understood as a comparative science of cultures or as the comparative analysis of social structures, Japanese anthropology today is only at its threshold. Such underdevelopment may be attributed partly to what I mentioned earlier, namely, the unique isolation and self-consciousness of the Japanese people. Moreover, what strikes me most is that in spite of the many anthropologists and anthropological works today, one rarely encounters theoretical originality. This is generally true of social sciences in Japan. This seems to me to be related to the dominant tradition of the *Kangakusha*, whose task was to master the imported ideas of the Chinese classics. It will take a long time to overcome the derivative character of Japanese intellectual life, for its roots are embedded in our history.

5. For example, see the bibliography (with introductory notes) in *Kinship and economic organization in rural Japan* by Chie Nakane, London, 1967, pp. 173–195.

14

Great Britain: Functionalism at home

*A question of theory**

ADAM KUPER

I. The intellectual tradition often termed 'British social anthropology' is as often summed up as 'functionalism', a broad, even rather meaningless designation which is nevertheless used by insiders as much as it is by outsiders. 'Outsiders' (cultural anthropologists as well as sociologists) tend to approach the tradition through the theories of the two functionalist masters, Malinowski and Radcliffe-Brown, thus compounding confusion. Malinowski's notion that culture is instrumental in satisfying man's biological and derived needs is contrasted to Radcliffe-Brown's neo-Durkheimian view that social institutions serve to meet the basic requisites of any continuing social order; and British functionalism is seen as a movement from the former to the latter position. Both approaches are sometimes regarded as complementary, essentially conservative philosophies of society or culture as constituting nicely fashioned mechanisms best left alone to continue their immemorial tasks.

Many are, of course, familiar with Malinowski's (1932, p. XXIX) famous remark that 'The magnificent title of the Functional School of Anthropology has been bestowed by myself, in a way on myself, and to a large extent out of my own sense of irresponsibility'. I wonder if Radcliffe-Brown's (1952, pp. 188–189) disclaimer is as familiar: 'I have been described on more than one occasion as belonging to something called the "Functional School of Social Anthropology" and even as being its leader, or one of its leaders. This Functional School does not really exist; it is a myth invented by Professor Malinowski.' At the least, such remarks should suggest that a certain caution is in order.

* This paper contains sections from a chapter of my book *Anthropologists and anthropology: British social anthropology, 1922–1972*, London, Penguin Books.

In fact, even Malinowski's most devoted followers showed a rather tepid enthusiasm for his explicit theories, and few of those most influenced by Radcliffe-Brown had much interest in the philosophical formulation of *A natural science of society*. 'Functionalism' – like 'structuralism' in the sixties – is best understood as connoting an intellectual mood, a method of approach and a judgment about appropriate models to emulate. It is perhaps an apt enough term for a tradition of work; but as a *theory* its various formulations are of secondary interest.

Modern British social anthropology staked out its claim in the twenties between the ethnologists, with their diffusionist proclivities, and the 'sociologists', who were, in Britain, concerned with the evolution of institutions. As Malinowski put it, in the concluding pages of his first major Trobriand monograph, *Argonauts of the western Pacific* (1922, pp. 575–576):

> Indeed, it seems to me that there is room for a new type of theory. The succession in time, and the influence of the previous stage upon the subsequent, is the main subject of evolutional studies, such as are practised by the classical school of British Anthropology (Tylor, Frazer, Westermarck, Sydney Hartland, Crawley). The ethnological school (Ratzel, Foy, Grabner, W. Schmidt, Rivers, and Eliott-Smith) studies the influence of environment on cultural institutions and race is studied by anthropologeography (Ratzel and others). The influence on one another of the various aspects of an institution, the study of the social and psychological mechanism on which the institution is based, are a type of theoretical studies which has been practised up till now in a tentative way only, but I venture to foretell will come into their own sooner or later. This kind of research will pave the way and provide the material for the others.

Despite his general combativeness, Malinowski always saw the new approach as essentially complementary to the more established tendencies within anthropology. Radcliffe-Brown was more uncompromising. In 1929 he wrote, 'I believe that at this time the really important conflict in anthropological studies is not that between the "evolutionists" and the "diffusionists", nor between the various schools of "diffusionists", but between conjectural history on the one side and the functional study of society on the other.'

Both the masters initially drew their inspiration from Durkheim, and the key dogma which both propagated was that the anthropologist should collect his material by direct observation and analyse it in its contemporary relationships. The issue of whether these relationships were 'functional' with respect to biology, psychology or the maintenance of

social order or were 'functions' in the mathematical sense was essentially secondary–meat for dispute between Malinowski and Radcliffe-Brown but not essential for the work of their students. In Malinowski's Trobriand monographs it is only the fact of connection between different kinds of activity that is of central interest. Canoe-building is connected (in practice) with magic, the mobilization of labor, etc. Procreation is connected with courtship, marriage, child-rearing, various biological beliefs, etc. The ethnographer must demonstrate these interconnections; he must show how the different aspects of human social behavior 'hang together' in action. In Radcliffe-Brown's analyses (as opposed to his programmatic statements) the essential task is to abstract the social relationships that are being expressed in customary behavior. Joking relationships express a relationship combining elements of conjunction and disjunction. Totemism is a mode of expressing group identity *vis-à-vis* other like groups. Customs cohere to form a system because ultimately all express the underlying social relationships which bind the members of the society together.

I am not concerned here with the theories of Malinowski and Radcliffe-Brown. The purpose of my paper is to discuss the functionalist tradition in British social anthropology, and this tradition developed in the monographic analyses by their students of particular exotic societies. The axioms of functionalism were straightforward; customs have a contemporary use and meaning, and we can make sense of even the most bizarre forms of institutionalized behavior if we place them in the living context of the ongoing life of the members of the society as social beings. Furthermore, neither of the masters offered more than very broad prescriptions. Faced with the increasingly complex field material which they brought back from a variety of societies and freed by the successes of their masters from the necessity of confronting the claims of the evolutionists and diffusionists, their students struggled with the problems of actually making sense of their data in these terms. A small, cohesive (if conflict-ridden) group, these students worked out their answers in the period 1930–1950.

II. Almost all of the first generation of anthropology students in the functionalist era passed through Malinowski's seminars at the London School of Economics. Even those who were attached to other universities made a point of attending. For the decade after 1924 this was the only school that counted.

Malonowski's dominance lasted from roughly 1924 to 1938. It was superceded by a sociological movement, led by Radcliffe-Brown. The shift is evident in the monographs of the period. There is a period of 'functionalist' studies in the thirties; then a period of transition; and, after 1940, a wave of neo-Radcliffe-Brownian studies. There is a time lag between the emergence of a new theoretical consensus and its public appearance in monographs. This is inevitable, given the length of time which elapses between the departure of the ethnographer for the field and the publication of his book. There is therefore some overlap, but the phases I have distinguished may nevertheless be identified with some precision.

A second shift – the move from the Pacific to Africa – at this time was not entirely the result of initiatives within the profession. Anthropologists now began to study large-scale societies, which were often difficult to demarcate geographically and which had complex political institutions. These new interests were to have a distinct influence upon theoretical developments, particularly after the publication of *African political systems* in 1940.

But beneath all this there remained the Malinowskian emphasis on field work by participant-observers. This became the hallmark of British social anthropology. The rules were clear: One had to spend at least a year, preferably two, in the field, working as soon as possible entirely in the vernacular, living apart from other Europeans and to some extent as a member of the community one was studying; and above all, the anthropologists had to make a psychological transference – 'they' had to become 'we'. Of course, not everyone kept all the rules. Indeed, it is now clear that Malinowski did not. Nonetheless, the guideline he laid down represented an ideal which his students strove to achieve.

III. The monographs of the thirties which are usually called 'functionalist' might more precisely be termed Malinowskian. They reflect his interests, even when the authors differed from him on particular points. They dealt with family life, economic activities and magic. They were not particularly concerned with kinship systems, politics or religion. Malinowski's example also permeated the presentation. The authors often included a topical biography – a development from the old-fashioned catch-all 'life history' presentation – and this helped them to integrate their descriptive material in a coherent but nonstructural fashion. The monographs moved from the particular institution along the whole

gamut of the culture, achieving a thematic integration, if not a more analytical coherence. Individual misbehavior was generally stressed, and the reader was introduced to particular characters and was offered evocative descriptions to give life to the culture.

The best-known monographs in this genre are Firth's *We the Tikopia* (1936), Richard's *Land, labour and diet in northern Rhodesia* (1939) and Schapera's *Married life in an African tribe* (1940). Fortune's *Sorcerers of Dobu*, published in 1932, dealt with the ethnographic region Malinowski had made his own, and, although he cannot be considered in the usual sense a student of Malinowski, this book is a good example of the kind Malinowski's students were writing.

Although studies of this sort continued to appear well into the forties, particularly in the outposts of the anthropological world, some British anthropologists were writing work of a different kind. For example, Schapera's *Handbook of Tswana law and custom* appeared in 1938, before his Malinowskian study of family life; it was a constitutional study of the sort which became familiar in the following decade. Nevertheless these Malinowskian studies belong to the 'thirties', and they typified the work being done in Britain at the time.

Both *We the Tikopia* and *Married life in an African tribe* were concerned primarily with the family. Like Malinowski in the *Sexual life of savages* their authors presented the family in terms of its 'functions', mainly procreation and socialization, and worked out from there to the implications of these processes for other aspects of the social situation. Audrey Richard's massive account of the Bemba treated the economy in terms of the simple, inescapable function of providing food. These are long, discursive books, and they would soon seem old-fashioned and undisciplined in their manner. *We the Tikopia* runs to almost 600 pages, and, because there is no theoretical framework of any substance, one wonders almost why the author stopped when he did. The authors did not abstract. Principles of social organization were shown immanent in concrete activities rather than as forming systems which could be comprehended. There is no notion of a kinship system in these early monographs of Firth and Schapera and no notion of an economic system in Richard's book.

The weakenesses were recognized. Firth (1962) has written of the late thirties: 'The basic problem raised by the functional theory of anthropology in its less sophisticated form – if everything is related to everything else, where does the description stop? – was much before the

writers of the period.'[1] The problem, in fact, was how to distinguish analytical relevance from empirical connection.

What was lacking was a theory which would specify what was relevant and what was peripheral to the resolution of a particular problem or to the understanding of an event. For example, Firth spent seven fascinating pages in *We the Tikopia* on sexual themes in conversation, humor and stories (in addition to a very valuable section on indecency). One of the Tikopian tales he related was that of a woman who tempted her husband's penis outside their house and threw it into the sea. She would scoop it up whenever she wanted sexual gratification, but one day her son came along, took the penis for a sea-slug and shot it dead with his arrow. Now this Tikopian Oedipus story may be relevant to all sorts of theoretical problems, but it was included only because it dealt with married couples and sex and so was connected to the theme (and presumably because it was entertaining in itself). It did not relate to an overriding issue but rather raised an irrelevant little problem of its own–something which would better have been treated in a separate paper. Just as the descriptions threw up these disconnected little problems, which gave the monographs a spurious theoretical content, so the material lent itself to moralizing, in the absence of a pervasive theoretical concern. Thus, Schapera gave weight to his descriptions by showing what a mess the missionaries were making of traditional Tswana morality, and Richards argued the point that the Bemba were not idle, as the colonists thought, but rather undernourished.

The field worker searching for criteria for abstraction and selection was forced into a consciousness of the absence of helpful theoretical orientations. This was the central issue in the anthropology of the late thirties.

In 1937 Radcliffe-Brown was setting forth his views at length in Chicago, but the value of the sociological option had still to be *demonstrated* in a social anthropological analysis of the sort of material the field workers were bringing home. The experimental works of this period were not exclusively sociological in their interests, but in their emphasis upon frameworks for abstraction they had a marked structural tone. In 1936 Bateson published *Naven*. In 1937 Evans-Pritchard's first book appeared, *Witchcraft, oracles and magic amond the Azande*, and Radcliffe-Brown's students in Chicago produced a collection of essays,

1. I am grateful to Professor Firth for permission to quote from this paper.

Social anthropology of North American tribes and Warner's study of the Australian Murngin, *A black civilization*. The Chicago studies provided the demonstration of RadcliffeBrown's methods which had formerly been lacking, and they had their influence in Britain. The other two books, perhaps, are more original, representing spontaneous reactions to the problems of analysis.

IV. Bateson was a Cambridge natural scientist, and he related (1936 [1958], p. ix) that 'Dr. Haddon first made me an anthropologist, telling me in a railway train between Cambridge and Kings's Lynn that he would train me and send me to New Guinea'. But Haddon (and Marett at Oxford) had got into the habit of sending their students to be trained by Malinowski for field work. Bateson found Malinowski's notion of function unacceptably ambiguous, but he saw the possibilities of his adaptive theory of culture. 'This method of approach is probably sound', he wrote (1936 [1958], p. 27), 'and its careful investigation might give a coherent system of anthropology allied to systems of economics based upon "calculating man".' This was the direction in which Firth later developed the theory. Bateson also came under Radcliffe-Brown's influence, and the master accepted his views to such an extent that he usually referred the question of the relation between culture and structural form to Bateson's analysis in *Naven*.

Naven opened with the problem of selection and abstraction. The first chapter began (p. 1): 'If it were possible adequately to present the whole of a culture, stressing every aspect exactly as appears in the culture itself, no single detail would appear bizarre or strange or arbitrary to the reader, but rather the details would appear natural and reasonable as they do to the natives who have lived all their lives within the culture. But this was not a practical solution. Malinowski and Doughty might in their different ways approach it, but some sort of abstraction was inescapable. Yet the process of selection and abstraction should not be allowed to impoverish the interpretation. The anthropologists must convey in their proper proportion not only the structural factors but also the emotional tone of the life, which Bateson called the ethos. *Naven* was an experiment with methods of analysis – 'it is an attempt at synthesis, a study of the ways in which data can be fitted together, and the fitting together of data is what I mean by "explanation"' (p. 281).

Bateson distinguished structure in Radcliffe-Brown's sense, meaning a pattern of relationships between persons, and what he called cultural

structure, the relations between the premises of a culture, which form a coherent logical scheme. There was also a third system, the relationships between the emotional needs of individuals and the details of cultural behavior, and between these and the emotional emphases of the culture as a whole. All three frameworks could and should be used to organize the data. But as Bateson developed his argument, he came to realize that the various analytical concepts were not somehow inherent in the observed data, as Malinowski and Radcliffe-Brown tended to believe. They were 'labels merely for points of view adopted either by the scientist or by the native'. As he later wrote (pp. 262, 281): 'The final climax of the book is the discovery, described in the epilogue – and achieved only a few days before the book went to press – of what looks like a truism today: that ethos, eidos, sociology, economics, cultural structure, social structure, and all the rest of these words refer only to scientists' ways of putting the jigsaw puzzle together.'

The problem on which Bateson experimented was posed by a bizarre New Guinea ceremony, called *naven*, which was monted from time to time for a sister's child who had done something praiseworthy. The ceremony involved transvesticism and other dramatic reversals of normal behavior. For example, the mother's brother of the person being honored dressed in grotesque female attire, offered his buttocks to his sister's son and acted the female role in a fantastic similitude of copulation with his wife. This was a classical sort of anthropological problem-an apparently absurd ritual which invited the question, What sense does this make? Bateson's 'explanation' involved the abstraction of the data onto the three dimensions of social relationships, cultural assumptions and emotional content. Each of these sets of data was shown to make sense – sociological, logical and psychological sense, respectively – and each element in the ceremony was dissected and set in each of these perspectives.

The analysis was richly suggestive and contained many seminal ideas, such as the notion of 'schismogenesis', basically a rule that oppositions are continually and dialectically heightened once begun. Bateson himself later developed this idea in his 'double-bind' theory of schizophrenia, which was adopted by R. D. Laing. It reappeared in a slightly different form within anthropology, in Evans-Pritchard's analysis of Nuer feuding and in Gluckman's analysis of conflict.

Nevertheless Bateson's book did not 'catch on' among the anthropologists. Partly, perhaps, this was because he was isolated from the

main group of the profession, first in Cambridge and later in the United States. But more fundamentally, he did not convince others because his empirical basis was questionable. He admitted frankly the ethnographic weakness of *Naven* (pp. 278–279): 'It is clear that I have contributed but little to our store of anthropological facts and the information about Iatmul culture which I have used in the various chapters does no more than illustrate my methods. Even for purposes of illustration my supply of facts is meagre, and I certainly cannot claim that my facts have demonstrated the truth of any theory.' This was too much for the British empiricists.

V. Like *Naven, Witchcraft, oracles and magic among the Azanda* was an attempt to make sense of the bizarre, but where Bateson teased out all the aspects of the institution he studied, like some more systematic and analytical Malinowski, Evans-Pritchard was concerned only with the set of premises of the culture, with what Bateson had called the 'eidos'. His problem was one of rationality – 'Is Zande thought so different from ours that we can only describe their speech and actions without comprehending them, or is it essentially like our own though expressed in an idiom to which we are unaccustomed?' (1937, p. 4).

This kind of question had been food and drink to anthropologists for a generation. Malinowski had developed a theory of magical behavior which asserted that mystical acts make sense in much the same way as more mundane technical procedures – they are attempts to shape the future, to cope with the unforseeable accidents which may ruin the most carefully tended garden, to set at rest the anxiety of the man who has done all he can in the usual way to ensure success in an enterprise but who still knows very well that success is uncertain. But beliefs in witchcraft posed a special problem. As Evan-Pritchard remarked, there are no readily apparent parallels in our culture, so we are not tempted to explain the unfamiliar, perhaps misleadingly, in terms of a cognate practice of our own.

Witchcraft, oracles and magic analyzed four complexes of mystical belief and action among the Zande of the Sudan: Witchcraft, witch-doctors, oracles and magic. These beliefs are related to one another and constitute a single, comprehensible and self-sustaining system. In Evans-Pritchard's view, they were mystical as distinct from empirical modes of belief and action because they depend upon objectively false assumptions about the existence of certain supernatural phenomena.

The book reads as though it is addressed to a skeptic whom Evans-Pritchard is trying to persuade that these beliefs are in some way reasonable. Several arguments were developed to this effect. First, Evans-Pritchard described how easily he himself adopted these ways of thinking and acting. Second, he pointed out that they are not articulated in the abstract or arranged into a developed theory but are simply invoked piecemeal in specific situations. Hence, the Zande can ignore some inconsistencies. But his most powerful argument, repeated at length and in various ways, was that given the initial premise that harm can be caused by mystical agencies, the notion that ill-luck took the form of a person, an evildoer, then the rest of the beliefs follow logically enough. Moreover, they are continually reinforced by experience. Somebody becomes ill, hence witches are active. Oracles confirm this. Vengeance magic is made. Somebody in the neighborhood dies, and the oracle confirms that he was the witch.

Therefore, Evans-Pritchard concluded (pp. 540–541):

> I hope that I have persuaded the reader of one thing, namely the intellectual consistency of Zande notions. They only appear inconsistent when ranged like lifeless museum objects. When we see how an individual uses them we may say that they are mystical but we cannot say that his use of them is illogical or even that it is uncritical. I had no difficulty in using Zande notions as Azande themselves use them. Once the idiom is learnt the rest is easy, for in Zandeland one mystical idea follows on another as reasonably as one common-sense idea follows on another in our own society.

But the monograph had a more combative purpose as well. Evans-Pritchard saw it as in part a challenge to Malinowski and as an example of the type of abstract argument which anthropologists should aim to develop. In the introduction to the book (pp. 2–3) he wrote:

> If any one were to urge that in discussing magic I have made a partial abstraction of the activities with which it is associated, I would reply that I am dealing with only some of its relations. It would be grotesque to describe Zande economic life in a book on Zande magic, oracles, and witchcraft, since agriculture, hunting, and collecting are not functions of these beliefs and rites, but the beliefs and rites are functions of agriculture, hunting, and collecting.

This evoked Malinowski's penchant for relating magic to every activity with which it was associated and his dogma that one cannot understand a custom except by describing all its concomitants. Further, in this passage he pointedly used 'function' in the mathematical sense of

something being a 'function of' something else and not in Malinowski's utilitarian sense.

Nonetheless the book in many ways reflected the theories of Malinowski and even an earlier orthodoxy. The problem of rationality was the problem of Tylor, Frazer and Lévy-Bruhl; and the institutional focus was typically Malinowskian, if the method of abstraction was not. Moreover at the heart of the book was an opposition between mystical and empirical beliefs and activities, an opposition which Evans-Pritchard took over from Frazer and Malinowski. He showed that the Zande do not make this contrast and that they believe mystical forces operate in much the same way as physical forces, but he retained the category opposition in his analysis. This weakened his critique of Malinowski's theory of magic. He wrote, for example (p. 439), 'We shall not understand Zande magic, and the differences between ritual behaviour and empirical behaviour in the lives of Azande, unless we realize that its main purpose is to combat other mystical powers rather than to produce changes favourable to man in the objective world.' It would have been more interesting to examine the total Zande theory of causation, without this extraneous dichotomy between 'mystical' and 'objective' forces. However, the dichotomy was implicit in the rationalist theme of the whole enterprise – to show 'us' that although 'they' believe in magic, they are still capable of logical thought.

The book was not concerned merely with explaining apparently irrational beliefs; it was also, very consciously, a model of abstraction. Many years later, Evans-Pritchard wrote (1951, p. 96): 'Abstraction can mean several different things. It can mean treating only a part of social life for particular and limited problems of investigation, taking the rest into consideration only in so far as it is relevant to these problems, or it can mean structural analysis through the integration of abstraction from social life.' He identified the first approach with the work of Malinowski and anthropologists like Margaret Mead in America. The structural method was the radical feature of *Witchcraft, oracles and magic*. It was developed to an even more severe level of abstract discourse in his second book, *The Nuer*, which was published in 1940.

These books lacked the almost debauched abandonment to experiment of *Naven*, but they proved to have a more immediate impact and also a more fructifying effect in the longer run. This was partly because they succeeded where *Naven* failed, by giving a convincing analysis of a body of data. Also, Evans-Pritchard was the Crown Prince, and he

was now reunited with Radcliffe-Brown, who had captured Oxford. In any case, the Zande book stimulated a number of later studies. The analysis of the closed system of thought was generally taken for granted, but the subsidary arguments of the book became the central themes of later studies. The key was Evans-Pritchard's remark that witchcraft provided the 'socially relevant cause' of misfortune.

VI. The experiments in abstraction by Bateson and Evans-Pritchard produced two of the most worthwhile monographs in social anthropology, but their methods were not taken over by others. In 1937, the year in which *Witchcraft, oracles and magic* appeared, Radcliffe-Brown took up the first chair in social anthropology at Oxford. Evans-Pritchard and Fortes worked in his department, and together they developed a type of analisis new to British anthropology. This was concerned with the social structure and dealt mainly with kinship and political systems. If one is seeking to identify the models for this work, one must look to the studies of the Chicago students of Radcliffe-Brown, but these were without the political dimension which came to be characteristic of the Oxford school, and they also lacked the firm basis of Malinowskian field work in functioning societies.

It is a measure of the narrow institutional basis of social anthropology in Britain at that time that when Radcliffe-Brown came to Oxford his only teaching colleague was Evans-Pritchard, who had joined Marett two years previously as Research Lecturer in African Sociology on a salary of £300. Fortes joined the department from 1939–1941, with the insecure title of 'Acting research lecturer in African Sociology'. Even after the war, when Evans-Pritchard took over the department in 1946, his staff consisted of himself, Fortes (by then a Reader) and a secretary-librarian.

There were then four centers of social anthropology in England – at the London School of Economics, University College London, Cambridge and Oxford. But social anthropology in the modern sense was firmly established only at the L.S.E. Now, in 1937, Oxford was taken over, and in due course – though only after the war – the other two departments were captured. Since the number of scholars and institutions involved was so tiny, the concentration of Radcliffe-Brown, Evans-Pritchard and Fortes in Oxford was of the greatest importance. This was particularly so at a time when anthropology at the London School of Economics

was entering a period of relative decline, following Malinowski's departure in 1938, and when, moreover, University College London was still pursuing its curious diffusionist hypothesis and Cambridge was still slumbering in the pre-Malinowskian era.

The new balance of power was significant only briefly, for the war soon intervened. Anthropologists were dispersed, most of them engaged in specialized services connected with intelligence and administration. But the brief partnership of Radcliffe-Brown, Evans-Pritchard and Fortes at Oxford produced a series of studies, dealing mainly with politics and kinship, which established a new paradigm. After the war British social anthropology picked up where they had left off.

In 1940 three major works in political anthropology appeared from this group: *African political systems*, edited by Fortes and Evans-Pritchard and with a preface by Radcliffe-Brown; and two monographs by Evans-Pritchard, *The Nuer*, and *The political system of the Anuak*, both of which dealt with societies lacking centralized government, in what was then the Anglo-Egyptian Sudan. These studies were followed by others, which explored further aspects of segmentary political systems and which also analyzed the person-to-person relationships of kinship within such societies. Although these were mainly written before and during the war, they appeared immediately afterwards – Fortes' two monographs on the Tallensi in 1945 and 1949; Evans-Pritchard's study of the Sanusi of Cyrenaica in 1949 and his book on Nuer kinship in 1951. These studies must be considered together. They form a coherent set, and they established the new paradigm which was to guide British social anthropology throughout the fifties.

If these studies are to be appreciated in their proper historical context, it must be remembered that they dealt with relatively novel problems which had emerged from the new wave of research in Africa. Seligman had carried out survey work in the Sudan, but the first modern studies in Africa, by participant observation, were carried out by Evans-Pritchard and Schapera. Evans-Pritchard's field work in the Sudan and Kenya was carried out between 1926 and 1938. Schapera spent 45 months with various Tswana tribes between 1929 and 1934. In 1930 Audrey Richards went out to study the Bemba, and then, with the first studies financed by the International African Institute, a number of others followed in the thirties.

These field workers were confronted not with tiny, bounded, island populations, but with comparatively huge, extended, dispersed tribes

and nations. It was quickly apparent that the sorts of social controls which writers on Oceania had identified – reciprocal obligations, exchanges, magical controls – formed only a small part of the governmental mechanisms of these societies. These problems were particularly urgent, since the colonial authorities were very much concerned with the methods by which these peoples could most effectively be administered, and, to the extent that Lugard's principle of indirect rule was adopted, some accomodation had to be made to 'traditional' forms of government. The most acute problem was posed by those societies which lacked centralized political institutions, and it was in the study of these societies – which fortunately included Evans-Pritchard's Nuer (though not the Azande) and Fortes' Tallensi (though not the Ashanti) – that social anthropology was to make perhaps its most original contribution to the social sciences and to political philosophy.

These factors directed attention to the political systems and social control. At the same time, the way in which these problems were worked out was modulated by theoretical developments within the discipline. In particular there was the new concern with models for abstraction, and the influential presence of Radcliffe-Brown.

VII. Yet these books also represented a departure from Radcliffe-Brown's position – the difference in tone, definition and emphasis between Radcliffe-Brown's preface to *African political systems* and the introduction by Evans-Pritchard and Fortes has often been commented upon. The new emphasis was, in a phrase, upon the lineage as a part of the system of political relationships rather than simply as a mode of organizing personal relationships, which was Radcliffe-Brown's primary conception. The term 'social structure' came to connote the structure of relationships between groups and, in Fortes' work, offices, rather than between persons.

In the introduction to *African political systems*, the editors distinguished two types of African polity – the centralized, pyramidal structure, exemplified by the Zulu, Tswana and others, and the 'stateless society'. The latter type was subdivided into band-type systems, such as that of the Bushmen, in which (the editors mistakenly believed) the political system was coincident with the kinship system, as Radcliffe-Brown had argued for the Australian aborigines; and the systems based upon segmentary lineages. The editors particularly emphasized the segmentary lineage systems, and one effect of *African political systems* was to divert

attention from the many stateless political systems in Africa which were not organized in terms of either kinship or lineage.

The emphasis in this seminal work was distinctly odd. Evans-Pritchard later explained (1958, pp. x–xi), 'the tentative typology Professor Fortes and I put forward ... was intended to be no more than a convenient start towards a more detailed classification of types of African society, in which the absence or presence of forms of descent groups and of state institutions were two criteria ...'. The presence or absence of state institutions was an obvious enough criterion, dictated as much by classical philosophy and anthropology as by the difficulties of colonial administration. But why should the presence or absence of descent groups be selected as a primary criterion for the classification of political systems? It is difficult to believe that it would be as readily selected today.

The emphasis upon the segmentary lineage systems had several sources. First, Durkheim had defined a broad class of segmentary societies, which were presumed to be typical of 'primitive societies' and which were thought to be based upon divisions of clan and territory. The evolutionary stage which intervened between the horde and state was characterized by a combination of the principles of kinship and territory – 'blood and soil'. Fortes and Evans-Pritchard simply took over this evolutionist classification and, as it were, stood it on its side. They did not present it as a classification of political systems in time but rather in space.

Third, in the study of lineage-based societies, the anthropological expertise in the study of kinship could be used as a bridge to the unexplored terrain of political systems. At the same time anthropology could escape from Radcliffe-Brown's tendentious equation of the kinship system and the social system.

But finally, and perhaps most important, there was the accident that both Fortes and Evans-Pritchard had chanced to study societies of this type and were engaged, together, in analyzing their material. *African political systems* set up a general classification, but the implications of their thinking were seen most clearly in their analyses of segmentary lineage-based systems and in particular in *The Nuer*, the most important and influential anthropological monograph of the period.

VIII. The thrust of the argument of *The Nuer* was to show that Nuer notions of time and space were a function of their social and economic values. Moreover, the social referents of these notions were not fixed

but varied with the social context in which they were formulated. In this way the analysis of these notions bridged the opening chapters of ecological and economic analysis, as well as the political analysis which was to come. First, the economic and physical conditions constrained the kinds of political and organizational responses which were possible. Second, Nuer values made a connection between ecological relationships of time and space and the structural relationships between groups. Indeed, the rest of the book was concerned primarily with demonstrating the way in which group relationships were conceptualized in territorial, *spatial* terms and in terms of lineage relationships, based on a genealogy stretching back *in time*.

Political relationships in Nuerland are basically territorial. The tribe is the largest political community, within which homicide should be settled by the payment of blood-wealth rather than vengeance. The tribal territory is divided into local units, which are again divided and subdivided. At every descending level of segmentation, the group is more cohesive and tends more readily to cooperate and to settle disputes amicably. The segments operate only in opposition to other like segments. If a man in one village killed a man in another, the two villages would mobilize to settle the debt. If a man in one of these villages killed a man in another district, the two villages would unite with other villages in their district against the villages of the other district. Evans-Pritchard termed these processes of division and coalition 'fission and fusion'. He wrote (1940, p. 148): 'fission and fusion in political groups are two aspects of the same segmentary principle, and the Nuer tribe and its divisions are to be understood as an equilibrium between these two contradictory, yet complementary, tendencies.' Alternatively, the structure could be understood as a balance of power at every level of organization.

The structure was expressed most fully in the blood feud. Although feuds were quickly settled between neighbors in the community of the village, who could not afford to set up a cycle of vengeance and counter-vengeance in the small cooperative group, they often led to violent confrontations when they involved members of more distant sections of the tribe. But any feud within the tribe could be settled by mediation and the payment of blood wealth. This mediation was usually through the good offices of a 'leopard-skin chief', a member of a hereditary group of mediators, respected but effectively powerless. Evans-Pritchard explained (1940, p. 159), in terms which recall Radcliffe-Brown's analyses

of opposition and conjunction: 'A feud has little significance unless there are social relations of some kind which can be broken off and resumed, and, at the same time, these relations necessitate eventual settlement if there is not to be complete cleavage. The function of the feud, viewed in this way, is, therefore, to maintain the structural equilibrium between opposed tribal segments which are, nevertheless, politically fused in relation to larger units.'

These territorial, political relationships were conceptualized in terms of the lineage idiom. Every tribe had a dominant clan, which was segmented into smaller patrilineal units – maximal, minor and minimal lineages. Everyone in the clan would claim patrilineal descent from the founding ancestor, *A*. Two of *A*'s sons (perhaps), *B* and *C*, would be the founding ancestors of maximal lineages. Each maximal lineage would in turn segment into minor lineages, and these into minimal lineages, with reference to a particular pivotal ancestor at an appropriate level of the genealogy. In some contexts, all members of the clan would identify themselves as *A*'s as against members of another clan. In other contexts, members of minor lineages *D* and *E* might unite as *B*'s against members of minor lineages *F* and *G*, who were *C*'s. And so on, thereby creating a pattern of fission and fusion, but one mapped in terms of lineage rather than territory.

This lineage framework worked at a level of values rather than crude demography. Members of one village might belong to various descent groups, and the members of the minimal-lineage segment of the dominant clan might be scattered, perhaps constituting only a minority of the inhabitants of a particular village. Nevertheless, when discussing their relationship with outsiders, the villagers would identify themselves with the minimal lineage of the dominant clan, using the clan's genealogy to express these relationships. Every level of lineage formation corresponds to a level of territorial grouping, from the clan which provided the identity of the tribe, to the minimal lineage, with a depth of only three to five generations, which provided the core of village unity.

As Evans-Pritchard showed, the lineage system worked in terms of genealogical time, the territorial system in terms of structural space. But the lineage framework provided a way of talking about the territorial system: 'The system of lineages of the dominant clan is a conceptual skeleton on which the local communities are built up into an organization of related parts, or, as we would prefer to state it, a system of values linking tribal segments, and providing the idiom in which

their relations can be expressed and directed (1940, p. 212). In this way the values of agnatic relationship gave cohesion to the system. 'In the absence of a chief or king, who might symbolize a tribe, its unity is expressed in the idiom of lineage and clan affiliation' (1940, p. 236). Because the lineage system was in this way a function of the territorial system, 'the lineages are in a number and structural position strictly limited and controlled by the system of territorial segmentation' (1940, p. 248). Moreover, it was often necessary to manipulate genealogies in order to adjust to the political realities, and so the 'lineage structure is twisted into the form of the political structure' (1940, p. 241).

IX. The originality and elegance of *The Nuer* are beyond dispute, and it still stands as the supreme example in social anthropology of successful abstraction in the analysis of a single society. Of course, the book did not come from nowhere. Evans-Pritchard adopted Durkheim's and Radcliffe-Brown's view of the character of segmentary societies, based upon mechanical solidarity, and like them he looked for order in the realm of shared values – Durkheim's 'collective consciousness'. The perception of the relativity of social groupings, the way in which they emerge in opposition to other, like groups, and also the contrast between the internal divisions and external unity of social groups were all important themes in Radcliffe-Brown's work. The notion of segmentary opposition was current, and Bateson had used it most creatively in *Naven*.

But *The Nuer* nonetheless successfully developed a complete model which was original as a whole, if its parts were taken from current ideas in anthropology. Its originality lies in part in the compelling nature of the model and its analytical power for the Nuer material. It also examplified a new point of view which characterized its stable-mates, *African political systems* and the later books and articles of Evans-Pritchard and Fortes. These works developed a political view of social structure, analysing social relations from the perspective of the total set of public, intergroup relations. At the same time, they distinguished this domain of public, lineage-based political relationships from the domain of personal, domestic, narrowly kinship relationships. In doing this they liberated anthropology from the interpersonal model of lineage relationships.

Naturally enough, *The Nuer* was subject to considerable criticism from the Malinowskians, and a similar undercurrent of criticism has continued throughout the years of its greatest influence. The criticism has

two prongs. One is that the field work was inadequate – understandably enough in the circumstances – and that therefore Evans-Pritchard did not really know what was happening on the ground. This led him to develop a highly idealized, abstract analysis, which did not allow for the machinations of calculating man.

This argument is not valid. Evans-Pritchard had done several years field work in the Sudan and among the Kenya Luo, who are related to the Nuer, before undertaking his Nuer study. He was therefore an experienced observer, as well as an exceptionally able one, working in a familiar country and with personal knowledge of related cultures. Furthermore the very arduous nature of his work may have forced him to grapple with Nuer life at a deeper level than anthropologists have commonly to plumb. As Evans-Pritchard himself said, many years later (1956, p. ix), 'I was a *ger,* what they call a *rul,* an alien sojourner, among them for only a year, but it was a year's relationship of great intensity, and the quality of a relationship counts for more than its duration'.

Evans-Pritchard's other books and papers on the Nuer contain such a range of ethnographic detail that it is clear that *The Nuer* operated on a high plane of abstraction from choice rather than necessity. The monograph must be understood in the context of a conscious effort to develop structural abstraction in ethnographic monographs. Fortes stated the program in the introduction to *The dynamics of clanship among the Tallensi.* He contrasted the description of 'whole cultures', à la Junod and the prefunctionalists, with descriptions of particular institutions – such as the *kula* or witchcraft – in the Malinowskian manner. Both these approaches were then set against the new kind of study of social subsystem which the Nuer and Tallensi monographs exemplified. These had the advantage that 'all studies in social structure or political organizations or economic structure, being concerned with the factors of social integration, are necessarily concerned with attributes of the whole society' (Fortes, 1945, p. viii).

The other, related critical reaction has a rather injured tone. It feels cheated in some way. Evans-Pritchard began *The Nuer* with a good, earthy account of ecology; in his next book he dealt with real persons in multifaceted, low-level interaction. Why were these concrete facts not used in *The Nuer?* Why sweep them under the beautiful, but distracting, structural carpet? This was to miss the point. *The Nuer* was an exercise in the abstraction of the social structure. One may abstract on various planes, and Evans-Pritchard chose the Durkheimian level

of the 'collective consciousness'. Moreover, the model he developed parallelled that of the Nuer themselves. It is no good complaining that Evans-Pritchard's model omits individual variation. He was concerned with values, arguing that political relations 'are best stated as tendencies to conform to certain values in certain situations, and the value is determined by the structural relationships of the persons who compose the situation' (1940, p. 137). One may argue about whether political relations are best stated in this way; but this is a legitimate way of approaching the subject, and there is no denying the illumination it provided in Evans-Pritchard's hands.

X. Despite the disruption caused by the war the decade of the forties was a remarkable one for British social anthropology. It began with the publication of *African political systems,* which established – together with the Nuer, Anuak and Tallensi monographs – the political perspective on social structure. It ended with the symposium *African systems of kinship and marriage,* published in 1950, which with *The web of kinship among the Tallensi* (1949) and *Kinship and marriage among the Nuer* (1951) marked a breakthrough in the study of kinship systems. The latter development depended upon the former.

Meyer Fortes was the most important figure in the development of kinship theory in this decade, next to Radcliffe-Brown himself. He (1969, p. 28) has credited Radcliffe-Brown with developing 'the idea of the synchronic system of kinship relations, focussed on Ego, as the centre of an arrangement of kindred, grouped lineally and collaterally by recognized categories of relationships expressed in the terminology'. In other words, Radcliffe-Brown was the first to see kinship systems as functioning structures. But as Fortes has pointed out, Radcliffe-Brown – and his American students – tended to regard the kinship system as equivalent to the social structure in tribal societies. This was to overemphasize the role of kinship, even in societies like those of the Australian aborigines. Moreover, Radcliffe-Brown – like Malinowski – was too ready to assume that kinship relationships derived from the inner core of family relationships. Both writers also laid too much stress on the interpersonal relationships of kinship. In their work, Fortes wrote (1969, p. 72), 'the kinship system is envisaged as a bilateral network of recognized dyadic relations radiating outwards from the elementary family'.

The missing factor in these writings was the external weight of legal and political arrangements, which shaped the kinship system together

with the domestic pressures generated within the family group. Fortes later wrote (1969, p. 49): 'It is my contention that the major advance in kinship theory since Radcliffe-Brown, but growing directly out of his work, has been the analytical separation of the politico-jural domain from the familial, or domestic domain within the total social universe of what have been clumsily called kinship-based social systems.' Evans-Pritchard's Nuer books and Fortes' books on the Tallensi demonstrated this dual context of kinship groups, domestic and political. The point was given concrete form by dividing the studies of Nuer and Tallensi social structure into two volumes. In each case the first volume dealt with clan and lineage relationships, the second with interpersonal relationships of kinship.

Thus the forties began with *The Nuer* and the demonstration of the political significance of lineages. They ended with *The web of kinship among the Tallensi* and the analysis of the interaction between external and internal constraints upon kinship relations. The work of Fortes and Evans-Pritchard derived recognizably from Radcliffe-Brown, but it represented only one of the possible developments of his theories. In fact, Radcliffe-Brown never fully grasped its implications. In his long introduction to *African systems of kinship and marriage* (1950), he still treated descent groups and other kinship corporations mainly from the internal point of view. But other contemporaries were mostly converted. A notable instance was Audrey Richards, one of Malinowski's closest associates, who demonstrated in her contribution to *African systems of kinship and marriage* the interaction of familial and politico-jural constraints upon Central African matrilineal systems in what was one of the finest examples of the new approach.

Finally, it is important to note that this movement was facilitated by the use of a more sophisticated notion of structure. Evans-Pritchard and Fortes accepted the logic of Bateson and moved beyond the naive realism of Radcliffe-Brown. Fortes wrote in 1949 (p. 56):

> When we describe structure we are already dealing with general principles far removed from the complicated skein of behaviour, feelings, beliefs, etc., that constitute the tissue of actual social life. We are, as it were, in the realm of grammar and syntax, not of the spoken word. We discern structure in the 'concrete reality' of social events only by virtue of having first established structure by abstraction from 'concrete reality'.

It was necessary to achieve such a level of abstraction – to escape from the narrow focus upon interpersonal relations – in order to make the

sort of distinction upon which their work depended, between external and internal domains, between intergroup and intragroup relationships, between politics and kinship.

XI. The movement of British social anthropology in the thirties and forties may be charted along various dimensions. There was the shift from the dominance of Malinowski and the L.S.E. to Radcliffe-Brown, Evans-Pritchard and Fortes at Oxford. Oceania, with its small, bounded, apparently simple cultures was displaced as the main area for field work by Africa, with its large, sprawling, and often highly differentiated societies. In this period, too, anthropologists first adopted and then abandoned the concrete, institution-based functionalism of Malinowski, experimented with various modes of abstraction and finally, adopted a sociological, structuralist position. Finally, there was a change in topical interest from the family, magic and making a living to political and kinship systems – from the interests of not only Malinowski but also Frazer and Westermarck, to the different concerns of Morgan, Maine Rivers and Radcliffe-Brown. These various movement were connected at one level, as should be clear by now.

The whole period may be followed best through its central monographs. These not only reflected the current views and preoccupations but also acted as experiments in understanding and in explanation. The most successful became models to imitate. For much of the fifties British social anthropologists of the postwar generation were content to repeat the experiments which Evans-Pritchard had conducted in *Witchcraft, oracles and magic,* and *The Nuer.*

The Malinowskian tradition remained alive, however, and the contradictions in the new structural approach, and also its limitations, soon led the more adventurous to explore other routes through the jungle, following paths along which some even met, to their surprise, with a faithful discipline of Malinowski, still making a living, slashing and burning in the bush. Taking the broadest view, the really remarkable feature of this period was simply the creative energy shown by a group of only two dozen odd, in less than two decades. The achievement of British social anthropology in the interwar years warrants comparison with the *Année Sociologique* school in its heyday.

References

Bateson, Gregory (1936), *Naven.* Cambridge. (References in the text are to the second edition, Stanford, 1958, which carried additional material.)

Evans-Pritchard, E. E. (1937, *Witchraft, oracles and magic among the Azande.* Oxford.

— (1940), *The Nuer.* Oxford.

— (1951), *Social anthropology.* London.

— (1956), *Nuer religion.* Oxford.

— (1958), Preface. In John Middleton and David Tait (Eds.), *Tribes without rulers.* London.

Firth, Raymond (1962), History of modern social anthropology. Preliminary draft presented at a conference on the history of anthropology, held in New York, 1962. Cyclostyled.

Fortes M. (1945) *The dynamics of clanship among the Tallensi.* Oxford.

— (1949) Time and social structure: An Ashanti case-study. In M. Fortes (Ed.) *Social structure.* Oxford.

— (1969) *Kinship and the social order.* London.

Malinowski Bronislaw (1922) *Argonauts of the western Pacific.* London.

— (1932) *The sexual life of savages,* 3rd edition with special preface. London.

Radcliffe-Brown A. R. (1929) A further note on Ambryn, *Man,* 29.

— (1952) *Structure and function in primitive society.* London.

15

Great Britain: Functionalism abroad

A theory in question

JACK STAUDER

The purpose of this essay is to counteract other accounts of the development of British anthropology, such as that given by Evans-Pritchard in his *Social anthropology* (1951), in which this development is cast in an overwhelmingly idealist light, in terms only of ideas. Such idealist accounts of the British anthropological tradition, like idealist histories of other scientific traditions, tend to ignore or de-emphasize the material conditions – political, social and economic – which set the context within which ideas and theories are held and science is practiced. It is my contention that the British anthropological tradition cannot be satisfactorily understood unless it is grounded in an understanding of the historical relationship of British anthropology to British imperialism. In this paper I wish to draw the main outlines of this relationship, in which three phases of British anthropology, the prefunctionalist, the functionalist and the 'dysfunctionalist', correspond roughly but significantly with three phases of British imperialism in Africa: The period of colonial conquest and extension of British rule; the period of colonial consolidation; and the period of colonial disintegration and African nationalist and urban unrest.

II. The institutional origins and early growth of British anthropology in the nineteenth century were closely linked to an interest in the possible practical value of anthropology as an applied science – a link which has been demonstrated by Conrad Reining (1962) and George M. Foster (1969, pp. 181–184). Members of the early anthropological societies in Britain were especially concerned with questions of race and slavery, great issues which agitated British society throughout the first two-thirds

of the nineteenth century. Anthropological journals of the time were filled with articles making recommendations on these questions. Anthropology was conceived by most anthropologists at the time as a science of race, not culture – in fact the two were confused – and most opinions, whether or not presented as 'scientific', featured assumptions and conclusions which were more or less racist in character. Missionary interests and slave-holding interests, both well represented in early anthropological circles, both took as their tenet the inferiority of the darker races of the world. What a later president of the Royal Anthropological Institute was to observe of James Hunt was probably true of most early British anthropologists – that they believed 'the Negro had his place in Nature, and it was the business of anthropologists to define that place' (Keith, 1917, p. 19).

With the British government and a host of private concerns engaged in constant fighting and trading with black people, attempting to missionize, rule and exploit them on many fronts in the Americas and Africa, the 'place' of black people in the world was an important practical question in imperialist endeavours in the mid-nineteenth century. Although it was scarcely to be left to scholars to decide a question involving profits and power relations, many early anthropologists nevertheless hoped that their researches might be of some utility to the interests involved in the expansion of European power around the globe. In an article written in 1866 entitled 'Race in legislation and political economy', an anonymous author declares: 'It is a most mistaken idea that Anthropology is purely speculative and abstract. It is, on the contrary, more intimately related than any other branch of science to the sympathies of humanity, and, we may add, the utilities and requirements of society. It enters into every question connected with religion, government, commerce, and culture, which are all more or less affected by racial endowment and proclivity' (quoted in Foster, 1969, p. 183).

The slavery issue died down in the 1870s when slaves in the Americas became poorly paid 'free' plantation labor and the slave trade was replaced by other, more 'legitimate' forms of exploitation in Africa. But the last two decades of the nineteenth century witnessed the acceleration and culmination of British imperialist expansion, particularly in the 'scramble' for Africa in which Britain succeeded in asserting against other European powers her claims to rule or 'protect' diverse territories with large native populations. During the same period, anthropology was attaining academic respectability in Britain (in the form

of university chairs), and some of its promoters hoped to ally the new science of man not with controversial popular causes, as had been the case in the pro- and anti-slavery debates earlier in the century, but with the science of good government, specifically the administration of colonial peoples. Prof. W. H. Flower, speaking as President of the Royal Anthropological Institute in 1884, declared that:

> The subject of ethnography ... is perhaps the most practically important of the various branches of anthropology. Its importance to those who have to rule – and there are few of us now who are not called upon to bear our share of the responsibility of government – can scarcely be overestimated in an empire like this, the population of which is composed of examples of almost every diversity under which the human body and mind can manifest itself ... it is absolutely necessary for the statesman who would govern successfully, not to look upon human nature in the abstract and endeavor to apply universal rules, but to consider the special moral, intellectual, and social capabilities, wants, and aspirations of each particular race with which he has to deal (quoted in Foster, 1969, pp. 184–185).

A survey of the 27 Presidential Addresses to the Royal Anthropological Institute from 1893 to 1919 shows that in 14, or over half, of these addresses the President raised claims regarding the practical uses to which anthropology could be put in serving the Empire. For example, in 1894 President Macalister urged that the Institute become a clearing house for information about foreign peoples and make this anthropological knowledge available for Imperial purposes of commerce and rule, thereby fulfilling a national want and contributing in a real manner to the consolidation of the Empire'. To accomplish this purpose, Macalister observed that the Institute would need financial help from the 'wealthier classes' (1894, p. 416). A year later, Macalister complained that the government had not yet seen the need to maintain an official department for the systematic accumulation of anthropological knowledge or to make an ethnographic survey of the Empire (1895, pp. 467–468).

Succeeding presidents of the Institute repeated the complaint. Despite the anthropologist's claims that he was of great practical value, neither government or business seemed interested, and little in the way of funds or official recognition seemed forthcoming. In 1903 Prof. Haddon, in his Presidential Address, declared again that 'part of the business of the science of Sociology is to provide data which can be utilized by the practical politician' (1903, p. 19). Haddon hoped that the government would recognize this, and that 'a full knowledge of the local conditions

and a sympathetic treatment of native prejudices would materially lighten the burden of government by preventing many misunderstandings, and by securing greater efficiency would make for economy. To look at it from the lowest point of view, even a slight frontier trouble means a direct expenditure for the local executive and a stagnation of trade which is very costly.' In short, said Haddon, 'it "pays" to study Ethnology' (1903, p. 20).

President Haddon's address, however, concludes in an almost bitter tone, as he reviews the singular lack of interest shown in anthropology by government. Haddon particularly resented the paucity of funds, government or private, available for anthropology, and the very few and poorly paid posts open for anthropologists at the universities. He cited the need for field work by professionally trained anthropologists, and the lack of funds for such field work (1903, pp. 21–22).

These complaints went largely unheeded in the following years; in 1910 British anthropologists were refused even a grant of £500 from the government to set up an 'Imperial Bureau of Anthropology' within the Royal Anthropological Institute (Ridgeway, 1910, p. 10). And as late as 1917 the President, Arthur Keith, could complain that despite the Royal Anthropological Institute's devotion to 'the Empire for which it has worked and is working', the government still showed no 'intelligent sympathy with either our efforts or our aims ... our rulers must be made to perceive the administrative value of anthropology' (1917, p. 29).

Thus around the turn of the century in Britain, anthropology found itself in a peculiar position. The British Empire was at its zenith, rapidly extending its effective rule over millions of new subjects. The period was marked by Victorian confidence in the application of science to achieve progress and profits. British anthropologists were publicly promoting their new science as a potentially invaluable tool to be used in the Imperial mission, to aid government and commerce and the advance of 'civilization'. Anthropologists were desperately courting government or private recognition and especially financial support for anthropology in its potential uses as they saw them. But government and wealthy benefactors were spurning these advances and seemed little interested.

This discrepancy between anthropologists' desire to serve British imperialism and the lack of support for anthropology by the British ruling classes is a phenomenon probably explained less by the shortsightedness of the ruling classes than by the shortcomings of anthropology at the

time, for, despite the assertions of anthropologists, the actual state of British anthropology at the turn of the century was such that in practice it was for the most part producing work of little or no possible use to colonial administrators, missionaries and traders. A survey of the contents of the *Journal of the Royal Anthropological Institute* in the years before 1920 can lead only to this conclusion. In the years before World War I anthropology in Britain was dominated by controversies between diffusionists and evolutionists, who held in common, however, an historical and often speculative approach that was primarily concerned with reconstructing the past of mankind. To this end, all sorts of bits of cultural and racial information were of interest, from art forms to paleolithic implements, from cranium measurements to pot-fabrics to legends and games and all variety of miscellaneous custom. Isolated institutions, beliefs and artifacts were recorded, usually out of their social and cultural context, and reports from the field by missionaries, administrators and travelers tended to stress the more bizarre and exotic of the customs they observed. Few professionally trained anthropologists had seen Africa or other colonized areas first-hand, and anthropologists at home were dependent for their data mainly on the reports of untrained observers engaged directly in the colonizing process. The position of these observers inclined them to look at native cultures unsympathetically, from the point of view of outsiders and masters. Their misconceptions and racist distortions were often published as contributions to ethnology, and the armchair anthropologists not only depended on this data but were themselves usually adherents of more or less explicit ideas of racial determinism (*cf.* Harris, 1968).

It is little wonder then that the picture anthropology gave of Africa up to the First World War was unreal.

> The reader of the ethnological literature of that time was under the impression that 'savages' were very different from Europeans, that they had queer if not repugnant customs, that they lived in a pre-logical world of curious superstitions, that their strange behavior – deemed a submission to instinctive impulses – was explainable only by a theory of racial inferiority, and that their ways of life were therefore inferior to 'civilized' ones. All ethnographic books were far from blunt in expressing these ideas, but of the writings of that time, most were more or less explicit in their assertion of these views. ...
>
> For [anthropologists], the 'savage' was an abstract concept; a culture was not a reality lived by a group but was made of separate items which were compared with similar items from another society; the distinction

> between race and culture was not clear. These conceptions, reflecting the level of the developing discipline of anthropology, account for the image Africanists then gave of traditional Africa. But again, that picture was just the one corresponding to the needs of the first stage of colonization. ...
>
> The partition of Africa into 'spheres of influence', military expeditions into the dark continent of 'cannibals', and establishment of colonial rule were made morally acceptable – even virtuous activities – since the colonized peoples were so different, so inferior, that the rules of behavior for intercourse with civilized peoples were obviously not applicable. Indeed, the 'savages' were considered fortunate to be put under the rule of a Western country, to be obliged to work, and to be forbidden to engage in their immoral practices. The colonial expansion required that a certain image of the nonliterate peoples be accepted by Western public opinion. On a more refined level, ethnology supported that picture (Maquet, 1964, p. 50).

Jacques Maquet (1964) and Marvin Harris (1968) have argued that it was the very distortions and lack of truth in nineteenth-century anthropology which made it useful during the early periods of imperial enterprise.

But if nineteenth-century ethnology was eminently suitable as an intellectual justification for colonial expansion, it was not at all well suited to the succeeding era of colonial consolidation and exploitation. Populations of Africans, once pacified, needed to be administered. Ideas about African culture and society which were grossly unreal or distorted were of no help in this second stage of colonialism; in fact, lack of proper understanding of Africans might well stand in the way of achievement of the British colonial goal of ruling with a minimum of trouble and cost and a maximum of stability and profit.

A few astute colonial administrators around the turn of the century realized that they needed better and more systematic information about the peoples they were ruling. These administrators took the initiative in demanding that anthropology be taught to colonial officers and others working in parts of the Empire, and special anthropology courses were subsequently set up at Oxford and Cambridge for officers on leave and cadets training for the colonial service (Foster, 1969, pp. 185–186). Sometimes administrators who showed an interest in anthropology were assigned to investigations involving the collection of ethnographic materials. A few had published detailed ethnographies or ethnographic surveys by 1920, for example, P. A. Talbot (1915) in Nigeria, Sir Claude Hobley (1910) in Uganda, Captain Dale (Dale and Smith, 1920) in Northern Rhodesia, Sir Harold MacMichael (1912) in the Sudan, and Sir Harry Johnston (1897, 1902–1904) in Central Africa and Uganda.

Also in a very few cases colonial governments had obtained the services of professional anthropologists, such as Northcote Thomas in West Africa and Prof. and Mrs. Seligman in the Sudan, to provide the colonial government with knowledge of the social organization and customs of particular peoples as a background for administration (Forde, 1953, pp. 843–846; Foster, 1969, pp. 186–188). This handful of persons, in addition to a few gifted missionaries such as Henri Junod (1913) and E. W. Smith (Dale and Smith, 1920), had begun to write a few relatively systematic and reliable accounts of African cultures by the 1920s.

On at least a descriptive level some of these early ethnographies achieved their design of providing the kind of information about a subject people's culture and society that could be of some use to administration and missionary work (*cf.* Forde, 1953, pp. 843–845; Evans-Pritchard, 1951, pp. 110–111). Some of the early ethnographers, a very few in the years before 1920, attempted to describe whole cultures rather than isolated traits and in so doing were the forerunners of modern British social anthropology. But while often exhaustively descriptive, their books lacked explanatory power; when necessary, they resorted to diffusionist or evolutionary hypotheses to explain particular customs for, although these ethnographers were primarily interested in the present life of the peoples they observed, they lacked any theory which could integrate their many observations, much less allow them to analyze their data sociologically. They lacked a theoretical framework appropriate to their purpose.

The Rev. E. W. Smith would speak of this shortcoming years later on his accession to the presidency of the Royal Anthropological Institute in 1934. He had co-authored with Capt. Dale, an administrator, a two-volume ethnography of the Ba-Ila of Northern Rhodesia, published in 1920 (Dale and Smith, 1920), and he recalls (1934, pp. xvi–xvii) that

> it was for severely practical purposes that we studied the life before us, magistrate and missionary working in perfect accord to understand the mind of the people that we might do our work with some efficiency. That in our ignorance of anthropological method our published work fell far behind the more rigorous standards of today is only too certain. We were groping after a better technique. I can see now that what we were really groping for was the functionalist method, which, to my mind, provides the best basis for a practical Anthropology.
>
> I have ventured to inflict these personal reminiscences upon you because Captain Dale and myself are representative of a great host of men who have been cast into a similar situation. And it is they, I think, who have been mainly instrumental in bringing into existence what Dr. Haddon calls

> the 'newer Anthropology'. This has, in other words, sprung into being as the result of pressure from the practical man whose stern experience made him realize the need of it.

There was indeed by the 1910s and 1920s a growing felt need in British colonial Africa for a 'newer anthropology' that would be adaptable to the practical requirements of British imperialism. And, as I have indicated, British anthropologists had constantly aspired to serve the Empire and by so doing to serve what they saw as the interests of anthropologists by obtaining funds and recognition. I have suggested that the major obstacle to this mutually beneficial alliance between colonialism and anthropology lay in the current state of anthropological theory and practice, or lack of it. The 'elder' anthropology, ethnology, diffusionism and evolutionism stood in the way of progress. The time was ripe for revolution.

The conscious element in the revolution has rightly been identified with the names of Malinowski and Radcliffe-Brown. But it was certainly Radcliffe-Brown who was the more conscious of what he was doing.[1] The circumstances of Radcliffe-Brown's and Malinowski's field experiences are well-known, as is the coincidence of the publication in 1922 of their pioneering functionalist studies, *Argonauts of the western Pacific* and *The Andaman Islanders*. Less well-known is the fact that the year these books were published, Radcliffe-Brown was teaching in South Africa. He had come there first as ethnologist to the Transvaal Museum, but in 1921 he was appointed to the new post of Professor of Social Anthropology at the University of Capetown where, it was announced, he would 'organize and co-ordinate research in Bantu Studies, and will give special lectures on modern methods of research, etc.' (*Bantu Studies*, 1921).

In the third number of the first volume of the South African journal *Bantu Studies*, issued in 1922, appeared an article by Radcliffe-Brown entitled 'Some problems of Bantu sociology'. In his opening paragraph (1922, p. 38), the author indicated that to his mind the key problem for 'Bantu sociology' was in fact the problem of colonialism:

> In Africa, more perhaps than in any other part of the world, social anthropology is a subject not of merely scientific or academic interest, but of immense practical importance. The one great problem on which the future welfare of South Africa depends is that of finding some social and political

1. I am indebted to Brian Turner of Columbia University for suggesting to me the relevance of Radcliffe-Brown to the issues I discuss.

> system in which the natives and the whites may live together without conflict; and the successful solution of that problem would certainly seem to require a thorough knowledge of the native civilisation between which and our own we need to establish some sort of harmonious relation. Every day the customs of the native tribes are being altered, by the action of the legislature and the administration, by the action of our economic system, through the teachings of missionaries and educators, and through contact with ourselves in innumerable ways; but we have hardly the vaguest ideas as to what will be the final results of these changes, upon the natives and upon ourselves. There seem to be some who optimistically trust to the action of the natural laws that regulate the social development of man; but the forces of history sometimes lead to progress, sometimes to disaster. And it does, at any rate, seem certain that if certain existing tendencies are permitted to go unchecked occasions of conflict such as the Zulu rebellion, Bulhoek and Bondelzwarts [African rebellions] will become increasingly frequent.

Radcliffe-Brown adds that 'in the establishment of the department of social anthropology in the University of Capetown this practical importance of the subject has been kept constantly in view, and the teaching and research are being organized on this basis' (1922, p. 38). Radcliffe-Brown qualifies this intent by stating that not the anthropologist but the administrator and legislator must be the ones to apply anthropology. It was the job of anthropologists, however, to provide the rulers with the scientific knowledge which the latter could put into practice. (This was later to become a standard position of British social anthropologists towards the application of anthropology.)

In his article Radcliffe-Brown goes on to make several other points: The importance of the study of kinship for understanding African society; the importance of fieldwork; and the superiority of the functionalist approach over an historical or ethnological approach. He explains that

> In dealing with the facts of culture or civilisation amongst primitive peoples who have no historical records there are two methods of explanation that we may adopt. The first may be called the ethnological method; it attempts, by the co-ordinated study of physical characters, language, and the various elements of culture, and with the help of such archaeological knowledge as is available, to reconstruct hypothetically the past history of a people in its main outlines. Such problems are very interesting. ... But interesting as it is, and important as its results may sometimes be, this ethnological method does not often provide, and does not seem likely to provide, results that will be of any assistance to the administrator or the educator in the solution of the practical problems with which he is faced. ... A theory that the Bantu peoples had their origin in a mixture of Sudanese negroes and Hamitic people somewhere in the neighbourhood of the great lakes,

> even if substantiated, would give little help to the missionary who is wondering what will be the effect on the moral life of a Bantu people of an attempt to get rid of the custom of lobola [brideprice] (1922, p. 39).

An alternative method of dealing with culture Radcliffe-Brown calls the 'sociological':

> The aim of this method is not to reconstruct the history of a people but to interpret their institutions in the light of general laws of sociology and psychology. If, for example, we investigate by this method the custom of lobola, we seek to determine the function of that custom, what essential or important relations it has with other institutions, what part it really plays in the economic, moral and religious life of the tribe, and to what important needs of the social organism it is related. Such an investigation thoroughly carried out would enable the anthropologist to foretell with some degree of certainly what would be general effects on the life of a tribe of an attempt to abolish the custom in question (1922, pp. 39–40).

It is important to note that the custom of bridewealth, *lobola*, was at the time under attack by missionaries and others in South Africa. Radcliffe-Brown observes that such assaults on native customs tend to weaken the fabric of native culture, and he feared that this tendency might eventuate in social disintegration and African revolt against white rule. From this concern he derives his interest in the institution of bridewealth and the customs of kinship and marriage surrounding it, including the relationship between mother's brother and sister's son – the subject of his best known article written during this period, 'The mother's brother in South Africa' (1924). Also noteworthy is the conclusion of the bridewealth controversy in South Africa: Within a decade the government, after hearing the testimony of anthropologists and others, enacted legislation to protect native institutions such as the brideprice.

In 'Some problems of Bantu sociology' Radcliffe-Brown came to the conclusion that 'the study of such problems, the sociological and psychological problems of native life, is certainly far more likely to lead to results of practical value to South Africa than the study of ethnological problems' (1922, p. 40). A year later, in 1923, Radcliffe-Brown returned to this argument in an address entitled 'The methods of ethnology and social anthropology', presented to the South African Association for the Advancement of Science. In this address he opens a full-scale attack on all the different approaches to culture current at the time: The psychological approach, the evolutionist approach, the ethnological or diffusionist approach. He criticizes their methodological weaknesses, their search for origins which are impossible to verify, their

lack of a truly scientific or inductive perspective and, in the case of the diffusionist method, the eschewing of generalization in favor of particularism.

But Radcliffe-Brown is not satisfied with methodological criticisms. To demolish the older kind of anthropology to make way for the newer, sociological and functionalist anthropology he proposes, he raises as a final argument the criterion of which kind of anthropology can best serve the interests of colonialism: 'Now while ethnology with its strictly historical method can only tell us that certain things have happened, or have probably or possibly happened, social anthropology with its inductive generalisations can tell us how and why things happen, i.e. according to what laws' (1923, p. 141). Radcliffe-Brown advances his faith that ultimately knowledge of such laws of social behavior will give men control over social forces and 'enable us to attain to practical results of the very greatest importance' (1923, p. 30). But even in the short run, he is certain that functionalist sociology can obtain immediate practical results.

> In this country [South Africa] we are faced with a problem of immense difficulty and great complexity. It is the need of finding some way in which two very different races, with very different forms of civilisation, may live together in one society, politically, economically and morally in close contact, without the loss to the white race of those things in its civilisation that are of greatest value, and without that increasing unrest and disturbance that seem to threaten us as the inevitable result of the absence of stability and unity in any society (1923, p. 142).

Stability and unity in society were, of course, the conditions which corresponded to the fundamental theoretical concepts on which Radcliffe-Brownian sociology was to be based, namely, the concepts of integration, equilibrium and solidarity. These were more than abstract concepts in the context of South African society, where a subject but increasingly rebellious African population outnumbered the white settlers trying to dominate and exploit them.

Radcliffe-Brown continues in relation to the racial confrontation in South Africa:

> Now I think this is where social anthropology can be of immense and almost immediate service. The study of the beliefs and customs of the native peoples, with the aim, not of merely reconstructing their history, but of discovering their meaning, their function, that is, the place they occupy in the mental, moral and social life, can afford great help to the missionary or the public servant who is engaged in dealing with the practical

> problems of the adjustment of the native civilisation to the new conditions that have resulted from our occupation of the country. Let us imagine the case of a missionary or magistrate who is wondering what are likely to be the results of an attempt to abolish or discourage the custom of *ukulobola* (bride-price). He may experiment, but he then risks the chance of producing results that he has not foreseen, so that his experiment may do far more harm than good. Ethnological theories as to the probable past history of African tribes will afford him no help whatever. But social anthropology, though it cannot yet provide a complete theory of lobola, can tell him much that will be of great help to him, and can set him on the path of enquiry by which he can discover more. ... The problem of how to get rid of the belief in witch-craft is another of the same kind, in which social anthropology can supply the administrator with knowledge and understanding without which it is very unlikely that he will be able to find a satisfactory solution of his practical problems...: (1923), pp. 142–143).

Radcliffe-Brown's examples of fields of practical interest for anthropology were not to be neglected by anthropologists. The first intensive study in an African society by a professional social anthropologist was initiated by E. E. Evans-Pritchard in 1927 among the Azande of the Sudan, at the behest of the colonial government and on the subject of witchcraft, an even more sticky problem for European administrators than the problem of bridewealth. Evans-Pritchard, however, had not completed his research among the Azande when he was asked by the colonial government to shift his studies to the political institutions of the Nuer. As Evans-Pritchard tells us in his introduction to *The Nuer*, he was reluctant to drop his research among the Azande; but as he was then directly employed by the colonial government, he complied with its request (1940, pp. vii, 7–9).

Thus the Nuer were not selected for study out of pure scientific curiosity. What was the urgency, the necessity of a study of the Nuer? Evans-Pritchard does not answer this question in his introduction. At a later point in his book, however, he provides information that enables us to understand why his services were needed:

> The truculence and aloofness displayed by the Nuer is conformable to their culture, their social organization, and their character. The self-sufficiency and simplicity of their culture and the fixation of their interests on their herds explain why they neither wanted nor were willing to accept European innovations and why they rejected peace from which they had everything to lose. Their political structure depended for its form and persistence on balanced antagonisms that could only be expressed in warfare against their neighbors if the structure were to be maintained. Recognition of fighting as a cardinal value, pride in past achievements, and a deep

> sense of their common equality and their superiority to other peoples, made it impossible for them to accept willingly domination, which they had hitherto never experienced. Had more been known about them a different policy might have been instituted earlier and with less prejudice.
>
> In 1920 large-scale military operations, including bombing and machine-gunning of camps, were conducted against the Eastern Jikany and caused much loss of life and destruction of property. There were further patrols from time to time, but the Nuer remained unsubdued. In 1927 the Nuong tribe killed their District Commissioner, while at the same time the Lou openly defied the Government and the Gaawar attacked Duk Faiyuil Police Post. From 1928 to 1930 prolonged operations were conducted against the whole of the disturbed area and marked the end of serious fighting between the Nuer and the Government (1940, p. 134f.).

Evans-Pritchard had entered Nuerland early in 1930 before the Nuer had been finally subjugated. In fact, 'a government force surrounded our camp one morning at sunrise, searched for two prophets who had been leaders in a recent revolt, took hostages, and threatened to take many more if the prophets were not handed over' (1940, p. 11).

The context in which Evans-Pritchard did his field work explains the resistance he encountered:

> When some Nuer were found they refused to divulge the whereabouts of nearby camps and it was with considerable difficulty that we located one. ... As every effort was made to prevent me from entering the cattle camps and it was seldom that I had visitors I was almost entirely cut off from communication with the people. My attempts to prosecute inquiries were persistently obstructed.
>
> Nuer are expert at sabotaging an inquiry and until one has resided with them for some weeks they steadfastly stultify all efforts to elicit the simplest facts and to elucidate the most innocent practices (1940, p. 11f.).
>
> It would at any time have been difficult to do research among the Nuer, and at the period of my visit they were unusually hostile, for their recent defeat by Government forces and the measures taken to ensure their final submission had occasioned deep resentment. ... When I entered a cattle camp it was not only as a stranger but as an enemy, and they seldom tried to conceal their disgust at my presence, refusing to answer my greetings and even turning away when I addressed them (1940, p. 11).

Evans-Pritchard recounts these personal hardships to apologize for his work being only a 'contribution to the ethnology of a particular area rather than ... a detailed sociological study' (1940, p. 15). Other than to explain this misfortune of his, he shows no interest in how Nuer were reacting to the colonial rule that was being imposed upon them. Except for the incidental passage quoted above, he does not describe

the British war against the Nuer, much less the nature of Birtish colonial policy and administration in the Sudan. Studying the colonial government was not a government anthropologist's job.

The Nuer is therefore mostly irrelevant to our understanding of colonialism, but that it was relevant to the colonialists' understanding of the Nuer is indicated by the fact of their support for Evans-Pritchard's work and their direction of his inquiries. Exactly how the intelligence Evans-Pritchard gathered was applied by administrators and police we do not know, since this was not a fit subject for study at the time; but the understanding which had been acquired through anthropological research about the Nuer political system, their values, their ecology, etc., would have a definite usefulness. Bombs, machine-guns and mercenaries were wasted trouble and expense if the Nuer could be dominated and administered 'peacefully', with force held in reserve. 'Peaceful' domination must rely on manipulation; and manipulation rests on knowledge. That this reasoning was not foreign to the colonial mind is indicated by Evans-Pritchard in the paragraph quoted earlier: 'Had more been known about them [the Nuer] a different policy might have been instituted earlier and with less prejudice' (1940, p. 134).

Similar situations existed elsewhere in Africa, although they did not always leave British anthropology the legacy of a classical monograph such as *The Nuer*. In southeastern Nigeria in the late 1920s the British government encountered widespread resistance to and rebellion against its attempts to impose direct taxation on top of direct rule. In the wake of the rebellion, the colonial government and private institutions sent a number of anthropologists to the area to study the Ibo people and to make recommendations as to how they might be more successfully ruled. Two women anthropologists were sent because Ibo women had led the rebellion. The foremost British anthropologist in Nigeria at that time, C. K. Meek, was also sent. Later he wrote (1937, p. xv):

> It will have been gathered that, in instituting anthropological inquiries in Iboland, the Government of Nigeria was not actuated by any academic or antiquarian interest, but by the purely practical motive of bettering the administration. This indeed has always been the Government's attitude towards anthropological research, namely, that it should serve as a handmaiden of administration, by throwing light on the history, relationships, organization, and thoughts of the people, and so providing data which would help the Government to make the fullest use of native institutions as instruments of local administration. There is a popular misapprehension that anthropology is the natural ally of reaction. ... Educated Africans are

> sometimes inclined to share this view, and mistrust anthropological research as a kind of henchman of 'Indirect Rule'... .

Not a 'henchman' but a 'handmaiden'!

The anthropologists sent to the Ibo did provide the data which helped the Government make 'the fullest use' of native institutions in adapting them as instruments of colonial rule. Similarly much if not most British social anthropology in Africa during the 1930s and 1940s was to be focused on political and legal institutions. Such an emphasis must be seen in the light of the strategy adopted by British colonialism to implement and maintain social control over the millions of people under its government in Africa. This strategy, which came to be known as 'indirect rule', avoided wherever possible the use of direct administration and direct coercion–though, of course, military force was always held in reserve. Due to overextended imperial commitments, and a necessity to economize manpower and finances in Africa, British colonial governments preferred to retain and utilize traditional political institutions. These were integrated into the colonial administration, and traditional political authorities were maintained as paid agents of colonial rule. But in African political institutions were to be adapted and used by colonial governments, it was imperative to understand what they were and how they worked; to his end, anthropology was often seen as critically necessary. As E. W. Smith noted (1934, p. xxii), 'the extension of Indirect Rule has been preceded by, and based upon, anthropological research'. Traditional African political systems differed from one another in many respects; therefore they all required detailed investigation to discover what particular forms they took in different societies, how each institution worked and how it could be adapted to the colonial system, who among the indigenous political leadership could be coopted and used, and how these men and the system as a whole could be best controlled and manipulated to ensure the social stability necessary for orderly domination and exploitation of the colony.

In the context of indirect rule, functionalist social anthropology seemed obviously superior to the older, ethnological approach, for the 'newer' social anthropology not only concentrated on identifying and describing the key social and political institutions of a subject people, but it also attempted to analyze how these institutions worked. Those concerned with problems of administration, and knowledgeable about anthropology, came to accept the validity of Radcliffe-Brown's claims on behalf of the practical superiority of

the functionalist method (see Lord Hailey, 1938, pp. 42–45; E. W. Smith, 1934, pp. xviii–xxv).

But if studies of political and legal institutions were to become the main area for functionalist research in Africa, we can understand from Radcliffe-Brown's perspective why studies of other fields of behavior, such as kinship and marriage, ritual and witchcraft, were also regarded as important for the maintenance of colonial rule. Customs such as bride-wealth, polygamy and witchcraft presented administrators and courts with endless problems. But also factors such as the incidence of witchcraft accusations, the stability of the family, the strength of traditional morality based on values of kinship and ritual sanctions were regarded by anthropologists and administrators alike as indices of social 'health' (the psychological analogy on which functionalism relied had always been a popular one in statecraft).

Traditional African societies were usually seen by the functionalists, as well as by the colonial governments, as being 'healthy' societies, well-balanced, well-integrated and maintaining an enviable degree of social control over their members. The British strategy of indirect rule had aimed at incorporating and preserving these integrated and stable units within an integrated and stable Empire. This intention, however, was progressively undermined by two other, contradictory features of British imperialism–by the racist assumption that European civilization was culturally superior to African civilization and destined to replace it and, more importantly in the long run, by the European drive to exploit Africa economically and the radical socioeconomic transformation this entailed in the lives of African people. With the assault by European missions and schools and legislation on African customs, and with the imposition of a colonial economy involving taxes in currency, production for export and the (at first forced) stimulation of migrant labor, the old tribal systems which the British had hoped to preserve as instruments of social control began inexorably to break down. Traditional patterns of behavior, and the social and ritual sanctions which governed them, were progressively weakened and abandoned in many areas. This process was accompanied by such new developments as the appearance and spread of millenarian or nativistic religious movements often anti-European in content, by an increase in witchcraft accusations in many colonial areas and by the formation of a restless semi-proletariat in towns where prostitution, drunkenness and crime had begun to flourish.

Astute colonialists, including foresighted anthropologists like Radcliffe-Brown, regarded these symptoms of social change with alarm. They saw the old tribal systems disintegrating and asked how they could arrest and control the forces which had been unleashed. Radcliffe-Brown confronted this problem in an address he gave in 1930 entitled 'Applied anthropology' (pp. 270–271, 277):

> An insufficiently integrated society will usually if not always suffer from moral unrest, and this may show itself in many different ways. An increase in the rate of suicide; an increase in neuroses or functional, as distinct from organic, nervous disorders; revolutionary political movements, formation of new religious sects, particularly when accompanied by forms of collective hysteria or excess of emotionalism; any of these may be symptoms of the lack of social integration. The increase of certain forms of criminality may also sometimes be due to this cause. Thus there is in my mind no doubt that the increase in native crime in South Africa, which has been very marked in the last twenty years, is almost entirely due to the disintegration of the native society... .
>
> When we undertake the control and education of a backward people such as an African tribe or the natives of New Guinea, we are attempting to produce or to direct changes in their social integration. Our task is to substitute for the existing social structure some other and more complex structure. If we destroy or seriously weaken the existing structure without replacing it by some other more effective, then we only produce general social disintegration with all its attendant evils... .
>
> For a satisfactory control of social change amongst such a backward people it is therefore necessary first of all to understand the existing social structure and know what functions the various institutions, customs, and beliefs have in relation to social integration. All changes that are taking place in native life, whether in the imposition of new laws, the suppression of existing customs, alterations of the economic life, or of religious belief, must be considered in relation to the process of social integration so that a real understanding of what is taking place and of what are likely to be the ultimate results of any change may guide the administrative officer in his endeavour to control the life of the people. The interference of the white man–administrative officer, missionary, trader, or labour-recruiter–almost inevitably tends to produce some disintegration of the existing structure, and if this proceeds more rapidly than the re-integration into a new structure we get a general disorganisation of the society... .
>
> Provided with the knowledge of what the culture is and how it works, and with a knowledge also of the changes that are already taking place or are inevitable as the result of outside influence, it would then be possible to formulate in detail a plan for an administrative and educational policy that would bring about a new and wider social integration.

Radcliffe-Brown definitely considered it the mission of anthropology to provide colonial agencies with the necessary knowledge about the

processes of social integration and change. The same concern, both theoretical and practical, can be seen in the work of other British functionalist anthropologists in Africa from the 1920s through the 1950s. Such concern might reflect the hopes, on the part of some anthropologists, that by protecting the social fabric from disruption they could spare the peoples they studied from some of the worst consequences of colonialism. But such conservatism can also be seen as ultimately protective of colonial rule itself, for the social health of native society was linked in the colonial mind with the political health of the British empire.

For example, Radcliffe-Brown, in the address quoted above, offers a clue as to the ultimate source of anxiety underlying his recommendations: 'One can feel quite certain that more knowledge of the nature of Indian culture and proper grasp of the laws of social integration would have prevented our long experiment with India from reaching its present unsatisfactory position' (1930, p. 279). In India at that time an organized movement for national independence had emerged as a definite threat to British rule. In Africa less organized rebellions against colonialism had also occurred and would soon develop into movements for national liberation. These anti-colonial struggles were an underlying source of worry to colonialists and anthropologists like Radcliffe-Brown who saw in them the final outcome of the tendencies they regarded as 'social disintegration'. The ultimate concern of these far-sighted men, it could be suggested, was to arrest the process of disintegration of traditional societies, not for the sake of the people living in them but in order to arrest the incipient process of disintegration of the British Empire itself.

III. In retrospect we see of course that Radcliffe-Brown's ambitions for anthropology were unrealistic. Neither social science nor enlightened administration could contain the forces set in motion by colonialism which would eventually undermine and overthrow the British Empire. But these developments, and the rapidity with which they would take place, were not clearly foreseen in the 1920s and 1930s, and Radcliffe-Brown's optimism about the potential applicability of a revolutionized, sociological and functionalist anthropology carried the opinions of those who counted. For many decades anthropologists had been claiming that they could be of practical service to government and business. But as I have pointed out, before the 1920s these claims were mainly ignored by the interests on whom they were pressed. During the late 1920s and into the 1930s, however, this situation changed fairly rapidly, and the

alliance long anticipated by anthropologists was established. British anthropology was suddenly and increasingly successful in gaining recognition and substantial funding from private and governmental sources.

For 1925 the Council of the Royal Anthropological Institute reported a momentous breakthrough. For the first time in the history of the association it had received large donations from outside sources: £1000 from the Carnegie Trust and $17,500 from the Rockefeller Trust (*RAI*, 1926, p. 3; 1927, p. 3 fnt). For 1926, the Council could report

> ... wide extension of the recognition of the importance of ethnological investigation as a basis upon which the government and regulation of the backward races of the Empire must be framed. In the recent Imperial Conference, the place of applied science in the administration of our dependencies was recognized for the first time, but it at once took a place in the proceedings commensurate with is importance ... the claims of Anthropology in its practical application are not likely to be overlooked, as is shown by the Report of the Undersecretary of State for the Colonies. ... (*RAI*, 1927, pp. 7–8).

The President of the Royal Anthropological Institute in 1929, Professor J. L. Myres, in his Presidential Address 'The science of man in the service of the state' reviewed the history of the struggle to finance applicable anthropology. Since World War I, he noted, changes had occurred: 'The grosser sorts of imperialism were discredited, but empires remained–indeed, the surviving empires had grown ... but the so–called 'backward races' were pushing forward with disconcerting brusqueness, in the halflight of their own ideas, or ideas which they supposed to be ours' (1929, p. 48). Hence, a new interest in applying scientific method and findings to administrative and 'interracial affairs'. Professor Myres concluded his address by noting with gratitude the Rockefeller benefaction made to the Institute. In 1931, during his continued presidency, the Rockefeller Foundation made a further grant of $10,500 to the Institute (*RAI*, 1932, p. vi). These grants were small in terms of Rockefeller disbursements, but they were unprecedentedly large sums in terms of the Institute's normal budget, which had always been less than £3000 a year before 1925, the bulk of this income coming from memberships and subscriptions to the *Journal*.

But a more important development for the future of anthropology in Africa occurred in 1926, with the foundation in London of the International African Institute. The Institute's aims were to bring about 'a closer association of scientific knowledge and research with practical

affairs' (Lugard, 1928, p. 2). The Institute was established with contributions and support from all the colonial governments in Africa, as well as from the British and French home governments, and from various missionary bodies (Hailey, 1938, pp. 51–52). On the governing board of the Institute sat a mixture of former colonial administrators, heads of missionary associations and eminent academics specializing in African studies. The first head of the Institute was Lord Lugard, famous not only as a soldier for his role in the 'pacification' of many parts of Africa, but notable also as an administrator and as the architect of the theory of 'indirect rule' predicted in British Africa.

The International African Institute was to play a leading role in social science research in British Africa. From the 1930s to the present it has financed the publication of many of the best ethnographies about African peoples, and its journal *Africa* has carried many of the most important anthropological articles about Africa. Many of these books and articles were closely related to colonial problems and 'practical affairs'.

That the Institute was able to succeed in playing such a vital role in stimulating and shaping anthropological research in Africa was due in no small part to the grant of $250,000 it received in 1931 from the Rockefeller Foundation (Rockefeller Foundation, 1931, p. 250). As a channel for Rockefeller money, the Institute began supporting anthropological field research by trained anthropologists, although the Rockefeller Foundation in addition set up other programs and made other grants outside the Institute to support anthropological research. Colonial governments also were increasingly employing or giving grants to anthropologists during the 1930s, but it was not until after World War II that the Colonial Office in the British government took over the financing of the majority of anthropological research in the African colonies. Thus the bulk of professional anthropological research in British Africa in the late 1920s and the 1930s was dependent on Rockefeller money in one way or another–money which of course partially derived from the profits of the growing Rockefeller financial and mineral holdings in Africa.

Rockefeller grants to British anthropology were only a small part of a multi-million dollar funding program, initiated in 1922, to develop the social sciences in the United States and internationally (*cf.* Horowitz, 1969a, 1969b, 1969c, for an analysis of how Rockefeller and Carnegie 'philanthropy' was used to shape education and social science to serve corporate capitalism). As the trustees of the Laura Spelman Rockefeller

Memorial (consolidated with the Rockefeller Foundation proper in 1929) wrote:

> In the autumn of 1922 the Trustees of the Memorial considered an extended memorandum outlining a possible program in the social sciences. Through the promotion of the social sciences it was felt that there would come a greater knowledge as to social conditions, a better understanding of social forces, and a higher objectivity in the development of social policy. It was felt that through the social sciences might come more intelligent measures of social control that would reduce such irrationalities as are represented by poverty, class conflict, and war between nations (1933, pp. 10–11).

It is clear that such an interest in rational social control coincided with Radcliffe-Brown's ideas as to the direction in which anthropology should develop. Whether or not the trustees of the Rockefeller Foundation had ever heard of Radcliffe-Brown, the 'newer' anthropology he was championing, with its emphasis on scientific applicability, stood to thrive in the disbursement of Rockefeller grants.

In any case it was in the late 1920s and early 1930s that anthropology in the British Empire began receiving in substantial sums the money which allowed the profession to be greatly expanded and allowed anthropologists to pursue foreign field work as a matter of course. Apparently anthropology had finally convinced colonial governments and capitalist benefactors of its potential practical uses to them. The struggle for acceptance which British anthropology had waged for more than half a century was successful.

However, the kind of anthropology which had succeeded was functionalist social anthropology. Ethnology, the historical approach, inquires into the origins of culture. This kind of anthropology did not succeed; in fact it hardly survived in Britain. This fact became increasingly evident in the kinds of anthropological research supported by the colonial governments and the International African Institute and the Rockefeller Foundation; it is also evident in the articles published in the journal *Africa* and in the books published by the IAI. This research and writing was increasingly, and finally overwhelmingly, social anthropological and functionalist in character. Other anthropological approaches were less and less funded, published or practiced.

This process, which was almost completed by the end of the 1930s, was accelerated by the concurrent sweeping success of sociological and functionalist theory in the universities; there the ethnological tradition died out rapidly as Radcliffe-Brown, Malinowski and their students took

teaching posts and then the important chairs in social anthropology in Britain and the Empire. Students received their training more and more exclusively in a strictly synchronic functionalist approach. A new orthodoxy had been established and was to dominate British anthropology until at least the end of the British Empire in the 1950s and 1960s. Ethnology, together with evolutionary and historical materialist approaches, were all virtually eliminated as academic alternatives.

Radcliffe-Brown, in his scathing attacks on ethnology and the historical approach in the early 1920s, had declared that nevertheless the older and the new anthropology could live side by side. But such was not to be the case, as resources and positions were in fact allocated to only one side. This one-sided allocation would determine the near monolithic character of a whole generation of British social anthropology.

The theoretical ascendancy of the functionalist approach, however, was not unvexed by growing problems stemming from developments in the colonial situation. As noted above, Radcliffe-Brown had advanced his theories of society as best qualified to aid in combating the trends toward the 'disintegration' of African societies under the impact of colonialism and its capitalist economy, and the growing resistance of African peoples to the colonial system. But functionalist theory proved increasingly inadequate as a practical approach to these problems. Even in the late 1930s its failures were becoming obvious, as masses of Africans had been uprooted from their traditional lives to work in the cities and mines and plantations outside of the supposedly stable, timeless and isolated societies the synchronic functionalist approach was best equipped to deal with.

Early attempts to place these new developments within a functionalist framework shored up by naive notions of 'culture contact' (e.g., Malinowski 1938, 1945; *cf.* Gluckman's critique, 1947) gave way to more sophisticated theories and empirical studies of social change that were forced to depart further and further from functionalist assumptions (e.g., the work of Godfrey and Monica Wilson). The study of social change was particularly accelerated by the foundation of the Rhodes-Livingstone Institute in 1938 in Northern Rhodesia. It was initially financed through a trust set up by various colonial governments and mining companies and administered by a board largely representing their interests. The Institute was intended to be 'a center where the problem of establishing permanent and satisfactory relations between natives and

non-natives may form the subject of special study' (quoted in Mair, 1960, p. 98). The 'permanent and satisfactory relations' envisaged were presumably not ones that would threaten the profits of the mining companies involved or the class and caste privileges of the white settlers and colonial officials who held political power in British Africa: these interests all depended heavily on cheap and docile 'native' labor. Therefore they were greatly shaken by the widespread African rebellions and strikes on the Rhodesian copperbelt in 1935. That Africa's first social science institute should be established a few years later in Northern Rhodesia, in the general vicinity of the largest Western investments in Africa (South Africa, the Rhodesias, Katanga) was probably not coincidental. A full account of the development of the Rhodes-Livingstone Institute, and of the extent to which the intentions of its founders and patrons influenced its research, is a study that remains to be done. But some of the most notable theoretical and practical work to come out of the Institute, work dealing with 'pluralism', 'tribalism' and migrant labor, has been sharply attacked by an African anthropologist for its pro-colonialist, pro-employer bias (Magubane, 1969a, 1969b).

In the years after World War II British colonial government was taking an ever more active role in supporting and guiding anthropological research in Africa. The handmaiden role of anthropology to government for the period 1943 to 1960 has been amply and proudly documented by Lucy Mair (1960), a leading proponent of applied anthropology at the time. Mair's survey of the 'practical' contributions of British anthropology to colonial administration and economic 'development' during this period, as well as the financial contributions made by government and private interests to anthropology, leaves no doubt that the British government and the various African institutes of social science succeeded in their aim – 'the organization of studies of practical interest to governments and business firms' (Mair, 1960, p. 98). Many colonial anthropologists probably thought of themselves as doing work that was primarily 'scholarly' rather than 'practical', but the distinction was not cut and driep in terms of relevance to colonial administrators. As Mair says,

> We should not put an account of the social structure of an African people in a separate category as a 'theoretical' study because it was not focused on some problem of special concern to the government of the day; although of course we should agree that many of the generalizations which such accounts make possible are of more interest to the theorist of society than to the practitioner of government. We recognize, too, that some studies bear more closely on problems of policy than others, and priority lists were

> drawn up with these in mind. Studies concerned with the changes currently taking place in African society have a prominent place among these (1960 p. 98).

The 'priority lists', as Mair explains, served 'as guides, however, both to the research institutes and to applicants for Colonial Research Fellowships, who know that a project which is on the priority list stands a better chance of a grant than one which is not' (1960, p. 98). Selective funding allowed British colonial interests to insure that most anthropological research in the colonies would be relevant to their purposes. But relevant information was not narrowly conceived:

> Both the Rhodes-Livingstone and the East African Institutes have done invaluable work of a type that can be described as 'mapping'. Under their auspices what were unknown regions, from the sociologist's point of view, have become areas with well-marked characteristics, about which theoretical generalization is both possible and profitable, and into which administrative officers, if they wish, can now venture with much better guidance than was available to their predecessors. ...
>
> This mapping work is indeed an indispensable preliminary to any studies that might be directed more closely to the problems of administration or economic development. The recognition of this fact led to the preparation by the International African Institute with the assistance of a Colonial Research grant, of the *Ethnographic Survey of Africa.* ... Forty-six volumes have thus far appeared (1960, p. 98).

Until the demise of British colonialism in Africa, British social anthropology there was closely geared to the social and political problems of the colonial system. If the basic monographs of traditional societies written by British anthropologists now appear irrelevant to our understanding of problems of social change and conflict, this appearance is misleading. Closer inspection reveals that most of these works, like *The Nuer*, did have a relevance within the British imperial enterprise. And in addition to studying traditional society, British anthropologists, including all the well-known functionalists, paid much attention to 'practical' problems and wrote numerous reports and articles and studies in the vein of applied anthropology for the colonial government and administrative and colonial journals, as well as for the anthropological journals (for references to these works, see Forde, 1953, pp. 862–865, and Mair, 1960).

Naturally many anthropologists developed sympathies for the people they lived with and studied. Some of them, as Kathleen Gough has noted, took the role of 'white liberal reformers' and tried to intercede

to protect the people they studied 'against the worst forms of imperialist exploitation' (1968, p. 13). Such attempts, however, did not basically challenge the imperialist system. As this was predicated on domination and exploitation, it was ultimately impossible for anthropologists to serve the interests of the people they studied by suggesting reforms. Though some anthropologists may have come to be critical of some of the policies and practices of colonialism, especially in its declining years, such attitudes were usually muted and must be weighed against the services anthropologists continued to render imperialism. In short, anthropologists were part of the colonial system; few if any of them are on record as having opposed this system with the perspective of helping Africans to overthrow it. In fact, probably most anthropologists, consciously or unconsciously, accepted unquestioned the imperialist framework and its concomitant myths assuming a harmony of interest between ruler and ruled, exploiter and exploited. The accepted their roles and privileges in the colonial system and allowed their work to be generally confined and directed by this system (*cf.* Maquet, 1964).

In the 1950s and 1960s the British colonial enterprise in Africa finally disintegrated. Nothing in the way of social science or enlightened administration or military power could provide enough social control to stop the historical forces operating throughout the African continent and around the world. African peoples except in southern Africa gained their formal political independence. Anthropologists seem to have accepted the decline of colonialism just as they had accepted its rise. They could continue to practice their profession in most African countries, in a political and social climate more complex and substantially changed. British anthropologists, short of funds, were soon greatly outnumbered by American social scientists in Africa, reflecting the rise of a newer imperialism with its own neocolonialist interests in Africa (see Africa Research Group, 1969, 1971).

If British colonialism and its component of administrative anthropology failed to retain control of Africa, they did not fail to leave a legacy there. The showpiece of British colonialism, Nigeria, had been constitutionally constructed and administered according to the most enlightened canons of indirect rule assisted by anthropological advice. After independence this colonially bequeathed structure collapsed in tragedy for the millions of Africans trapped within it (see Diamond, 1966). Similar events have occurred in the Sudan and other African countries after independence. An anthropology which assisted colonial

governments in adapting African societies to European rule must share the historical responsibility for that rule and its aftermath. That is to say, the British anthropological tradition, above all others, has been 'the child of Western imperialism' (Gough, 1968, p. 12).

References

Africa Research Group (1969), African studies in America: The extended family. ARG, P. O. Box 213, Cambridge, Mass. 02138.

— (1971), Africa report. ARG, P. O. Box 213, Cambridge, Mass. 02138.

Bantu Studies (1921), Appointments, *Bantu Studies*, 1 (1): 10–11.

Dale, Andrew Murray, and Smith, E. W. (1920), *The Ila-speaking peoples of northern Rhodesia.* London, Macmillan and Co., Ltd.

Diamond, Stanley (1966), *Nigeria: Model of a colonial failure.* New York, American Committee on Africa.

Evans-Pritchard, E. E. (1940), *The Nuer.* Oxford, Oxford University Press.

— (1951), *Social anthropology.* London, Cohen and West Ltd.

Forde, Daryll (1953), Applied anthropology in government: British Africa. In A. L. Kroeber (Ed.), *Anthropology today.* Chicago, University of Chicago Press. Pp. 841–865.

Foster, George M. (1969), *Applied anthropology.* Boston, Little, Brown and Co.

Gluckman, M. (1947, Malinowski's 'functional' analysis of social change, *Africa*, 17: 103–121.

Gough, Kathleen (1968), Anthropology and imperialism, *Monthly Review*, April: 12–27.

Haddon, A. C. (1903), Presidential address: Anthropology, its position and needs, *Journal of the Anthropological Institute*, 33: 11–23.

Hailey, Lord (1938), *An African survey.* London, Oxford University Press.

Harris, Marvin (1968), *The rise of anthropological theory.* New York, Thomas Y. Crowell Co.

Hobley, Charles William (1910), *Ethnology of the Akamba and other East African tribes.* Cambridge, Cambridge University Press.

Horowitz, David (1969a), The foundations (charity begins at home), *Ramparts*, April.

— (1969b), Billion dollar brains, *Ramparts*, May.

— (1969c), Sinews of empire, *Ramparts*, October.

Johnston, Sir Harry (1897), *British Central Africa.* London, Methuen and Co.

— (1902–4), *The Uganda protectorate*, 2 vols. London, Hutchinson and Co.

Junod, Henri (1913), *The life of a South African tribe*, 2 vols. Neuchatel, Imprimerie Attinger Freres.

Keith, Arthur (1917), Presidential address: How can the institute best serve the needs of anthropology?, *Journal of the Royal Anthropological Institute,* 47: 12–30.

Laura Spelman Rockefeller Memorial (1933), Final report. New York.

Lugard, Sir F. D. (later Lord) (1928), The International Institute of African Languages and Cultures, *Africa,* (1): 1–12.

Macalister, A. (1894), Anniversary address, *Journal of the Anthropological Institute,* 23: 400–416.

— (1895), Presidential address, *Journal of the Anthropological Institute,* 24: 452–468.

MacMichael, Sir Harold Alfred (1912), *Tribes of Northern and Central Kordofan.* Cambridge, Cambridge University Press.

Magubane, B. (1969a), Pluralism and conflict situations in Africa: A new look, *African Social Research,* 7: 529–553.

— (1969b), Some methodological and ideological problems in the study of social change in Africa, as reflected in the studies of migrant labour. Paper delivered at the University Social Sciences Council Conference, Makerere Institute of Social Research, Kampala, December 30, 1968 to January 3, 1969.

Mair, Lucy (1960), The social sciences in Africa south of the Sahara: The British contribution, *Human Organization,* 19: 98–107.

Malinowski, B. (1938), Methods of study of culture contact in Africa (Memoranda of the International African Institute, no. 15.) London.

— (1945), The dynamics of culture change: An inquiry into race. In P. Kaberry (Ed.), *Relations in Africa.* New Haven, Yale University Press.

Maquet, Jacques J. (1964), Objectivity in anthropology, *Current Anthropology,* 5: 47–55.

Meek, C. K. (1937), *Law and authority in a Nigerian tribe.* Oxford, Oxford University Press.

Myres, J. L. (1929), Presidential address: The science of man in the service of the state, *Journal of the Royal Anthropological Institute,* 59: 19–52.

Radcliffe-Brown, A. R. (1922), Some problems of Bantu sociology, *Bantu Studies,* 1 (3): 38–46.

— (1923), The methods of ethnology and social anthropology, *South African Journal of Science,* 20: 124–147. (Reprinted in M. N. Srinavas [Ed]., *Method in social anthropology.* Pp. 3–38.)

— (1924), The mother's brother in South Africa, *South African Journal of Science,* 21: 542–555.

— (1930), Applied anthropology, *Report of the 20th Meeting of the Australian and New Zealand Association for the Advancement of Science, 1930.* Pp. 267–280.

Reining, Conrad C. (1962), A lost period of applied anthropology, *American Anthropologist,* 64: 593–600.

Ridgeway, William (1910), Presidential address: The influence of environment on man, *Journal of the Anthropological Institute,* 40: 10–22.

Rockefeller Foundation (1931), Annual Report. New York.

Royal Anthropological Institute (1926), Report of the Council for the year 1925, *Journal of the Royal Anthropological Institute,* 56: 3–8.

— (1927), Report of the Council, *Journal of the Royal Anthropological Institute,* 57: pp. 3–8.

— (1932), Report of the Council, June 1931 to June 1932, *Journal of the Royal Anthropological Institute,* 62: iii–vi.

Smith, Rev. Edwin W. (1934), Presidential address: Anthropology and the practical man, *Journal of the Royal Anthropological Institute,* 64: xiii–xxxvii.

Talbot, Percy A. (1915), *Women's mysteries of primitive people.* London.

PART III

Towards a critical anthropology

16 The Manchester School in Africa and Israel: A critique*

T. VAN TEEFFELEN

Among British social anthropologists the late Max Gluckman has occupied a unique place. Being a charismatic and influential personality, he gathered around himself a circle of younger anthropologists from the 1940s on, still commonly known as the Manchester School. Sociologically, their work is worth studying for two distinct reasons:

First, because the scholars involved shared a common paradigm, both theoretical and methodological. While working predominantly within the structural-functionalist framework, they gave it a special flavour by focusing on down-to-earth conflicts, breaches of rules, contradictions within and between norms. In brief: how people "really" behaved and how they used their institutions. Modes of presentation were shaped to such ends. The fundamental ideas were developed by Gluckman and he took great care, e.g. in his many prefaces to the books of his students, to situate their work in the tradition that he had established.

Fieldwork has been carried out in mainly two regions: southern and central Africa (in particular, the former North Rhodesia and Nyasaland) and Israel[1]. In both cases Gluckman for some time supervised two weighty research programmes: in North Rhodesia as director of the Rhodes-Livingstone Institute (from 1942 to 1947) and in Israel as director of the so-called Bernstein Scheme (from 1965 until his death, in 1975). Politically and ideologically both situations were at least problematic, and here and there perhaps suspect; this makes it particularly urgent to study the societal context in which Manchester theory and methods have been applied.

*Previously published in *Dialectrical Anthropology* 3 (1978) 67–83

1. Noted exceptions were Frankenberg, Peters and Worsley.

THE PARADIGM

The main publications of the Rhodes-Livingstone anthropologists can be logically ordered into three successive stages of investigation, respectively: the search for order and system, the analysis of actual behaviour of people (especially with regard to 'conflicts' and other disharmonies); and the way back to the notion of an integrated system.

The search for order was, as is well known, shared with other British anthropologists. Order was opposed to *chaos,* the latter term implied the notion of anarchism, defined as the lack of authority. The confrontation with 'stateless', more generally, classless and/or nonhierarchically structured societies intensified the problems of identifying with the subject of study. Thus Colson gives the following impression of the Plateau Tonga:

> On the whole the Tonga might be defined as culturally a have-not group. They have never had an organized state. They were unwarlike and had neither regimental organizations nor armies. They were and are equally lacking in an age-grade set-up, secret societies, and social stratification of all kinds. The Tonga would not even attract those fascinated by the intricated rules of lineage organization, for while they have clans and smaller matrilineal kingroups, they have them in a characteristically unorganized fashion which leaves the investigator with a baffled, frustrated desire to rearrange their social structure into some more ordered system.[2]

Colson seems to identify 'culture' with the presence of formal stratification. Van Velsen[3] responded similarly to the Lakeside Tonga: he sought "a few clearly discernable structural regularities as starting points in his search for order in this apparent chaos". But it seems that his search was hindered by other characteristics of the region: the matrilineal organization of many tribes (with its inherent conflicts between the "rights" of the matrilineality and those of the father) and, of course, the colonial setting with its recurrent violent breaks of order. However, because people were not involved in permanent conflicts, the presence of some order was assumed. Even in the Copperbelt towns of North

2. E. Colson, *The Plateau Tonga of Northern Rhodesia* (Manchester: Manchester University Press, 1962), p. 84.

3. J. van Velsen, *The Politics of Kinship: A Study in Social Manipulation among the Lakeside Tonga of Nyasaland* (Manchester: Manchester University Press, 1964), p. 312.

Rhodesia, where colonial conflicts were most manifest, an integrated system was pesent, accorditng to Gluckman (reviewing the work of A. L. Epstein and Clyde Mitchell):

> These studies show that we can find plenty of systematic regularities in the new African towns. These regularities are obvious in that people live and go about their business within the towns in relative security and absence of fear. Hence clearly there is some kind of working, integrated social system in these towns.[4]

While one may doubt this "relative" security and absence of fear, the Manchester anthropologists indeed took colonialism into account as a social system,[5] most obviously when they studied contacts between chiefs and the colonial administration or white-black relations in the Copperbelt towns. However, colonialism was seen from the viewpoint of stability and efficiency. In general, the choice of neatly delimited spatial units (tribe, village or city) had preference to that of historically oriented, and untidy topics of research which might not be particularly bound to one area or place – in accordance with normal anthropological practive at the time.

Having selected some structuring principles within the unit of study (usually, because of the anthropologist's western background, the result of a quite strenuous effort), the next step in the investigation concerned the ways in which these principles operated in real life. This turned out to be the most productive aspect of the Manchester approach. While each researcher selected a different topic (the range was wide, from politics and kinship to ritual and sorcery), the analytic procedure was common: try to discern the concrete strands by which people are tied to each other and analyze the conflicts and cleavages within this web. The ultimate aim was the development of one framework that covered both norm and reality, constraint and freedom, rule and exception. A whole realm of concepts was put forward: situational selection of norms and ties, manipulation of norms, social field, and of course

4. M. Gluckman, "Anthropological Problems Arising from the African Industrial Revolution," in A. Southall (ed.), *Social Change in Modern Africa* (London: Oxford University Press, 1961), p. 76.

5. Gluckman regarded I. Schapera as an early protagonist of the view that both Whites and Blacks had to be studied within a single social system. See M. Gluckman, "Anthropology and Apartheid: The Work of South African Anthropologists," in M. Fortes and S. Patterson (eds.), *Studies in African Social Anthropology* (London: Academic Press, 1975), p. 24.

the network concept. Modes of ordering the data were adapted to meet these research interests: well-known methods such as the 'extended case-study' and 'situational analysis' were used to dramatize the inherent conflicts of a social system as they became apparent in concrete circumstances. This approach implied a departure from formalist structural-functionalism by taking into account the daily deliberations of the people themselves. (Insofar as these were involved with 'petty politics').

However, the analysis did not stop at this point. At least during the period of frank colonialism, the original question 'how did the system work' was again taken up, but now in the context of the empirical findings: how could the system survive despite the conflicts and cleavages? Gluckman took the lead in answering the question by redefining the rigid system notion: no system could be considered tight, closed or self-contained and unchanging;[6] and distinguished between 'repetitive' and 'changing' systems. Finally he pushed the analysis to its triumphant end: the system survived not *despite,* but *because* of all conflicts and cleavages.[7] This last argument, which was shared by his pupils in Africa, and which would remain the 'trade mark' of the Manchester School, became evident in different forms: – conflicts on a lower level of social organization contribute to unity on a higher level, and thus actually strengthen the overall integration of the group. This analysis was paradigmatically stated in Gluckman's contribution to *African Political Systems* where he described pre-colonial Zulu society in terms of a segmentary tribal opposition that produced equilibrium on the higher level of the kingdom.

The theory of 'cross-cutting ties' was applied on a general theoretical level; Colson converted this notion into a philosophy of society:

> Tonga society, despite its lack of political organization and political unity, is a well-integrated entity, knit together by the spread of kinship ties from locality to locality, and the intertwining of kinship ties within any one locality. It obtains its integration and its power to control its members and the different groups in which they are aligned, by the integration of each individual into a number of different systems of relationships which overlap.

And she concludes, after a comparison of Tonga and Tallensi organization:

6. M. Gluckman, *Analysis of a Social Situation in Modern Zululand,* Rhodes-Livingstone Paper No. 28 (1958).

7. M. Gluckman, *Custom and Conflict in Africa* (Oxford: Basil Blackwell, 1955).

> The Tonga and the Tallensi are very differently organized, but the same principles of cross-cutting ties appears in both societies. I suspect that it is a general principle incorporated into most societies as a mechanism for ensuring the maintenance of order.[8]

Conflicts, as for example, expressed in ritual ceremonies, purify the participants and temporarily purge the society.

Gluckman came to see ritual

> not simply as expressing cohesion and impressing the value of society and its social sentiments on people, as in Durkheim's and Radcliffe-Brown's theories, but as exaggerating real conflicts of social rules and affirming that there was unity despite these conflicts.[9]

Apart from these three typical instances, a number of other variations on the theme were applied (e.g. various kinds of conflicts might lead *in time* to some form of integration). This particular conclusion was the ultimate consequence of fitting conflict theory into static structural-functionalism; while commonly held among Manchester anthropologists, it is, however, not wholly representative of their efforts. Although the vague notion of order was never completely left (at least during the colonial period), various authors, Gluckman himself being among the first, repeatedly declared their scepticism with regard to the capacity of various tribal or colonial systems to incorporate all the conflicts which were generated by them. The general question 'how is order possible' was turned into a more specific question: 'how much conflict can a system contain before it disrupts'. Integration and order should be conceived as gradual matters; order was not sel-explanatory, but should be critically tested on its *strength.* These questions led to an approach in which certain concepts such as the 'viability' or, indeed, the 'virility' of a social system fluorished. The focus became the dynamic viability or strength of the system instead of its merely static integration. However, the main work of the Manchester School in Africa was directed towards the analysis of the strength of the *status quo,* i.e. tribal systems and Indirect Colonial Rule, not towards the analysis of nationalism. Here they shared

8. E. Colson, *op. cit.*, pp. 120–121.

9. M. Gluckman, *Order and Rebellion in Tribal Africa* (London: Cohen and West, 1963), p. 18; quoted in A. Kuper, *Anthropologists and Anthropology: The British School 1922–1972* (London: Allens Lane, 1973).

the conservative bias of earlier work in the functionalist tradition.[10] Indeed, those research topics which the Rhodes-Livingstone fellows jointly tackled – the position of the tribal chief as intermediary between the 'Administration' and the Africans,[11] the Copperbelt cities as social systems,[12] and the effects of migratory labour[13] – were crucial tests of the capacity of the overall colonial system to cope with the fundamental conflicts of the time under the aegis of Indirect Rule.

The separation of values and science

Politically, most of the anthropologists concerned were liberal reformists. Kuper writes:

> As a group – with a few exceptions – the Rhodes-Livingsotne fellows were politically on the left, and not backward showing it.[14]

I think the addition is somewhat exaggerated: with the occasional exception of Gluckman (especially on South Africa) none of the anthropologists was inclined to ventilate his or her political views – if one did so, it occurred implicitly, in stressing, for example, the necessity of 'common understanding' or similar general advice.[15] In line with the theoretical bias, the colonial context was not questioned. Allusions to differences in interest among the Administration, the settlers, and the African population were only vaguely made, without elaboration. On the other hand, commonality of interests was stressed, as for example, by van Velsen:

10. Especially A. Gouldner, *The Coming Crisis of Western Sociology* (London: Heinemann, 1970), p. 125ff., on 'the problem of anthropology and sociology in England.'

11. J. Barnes, M. Gluckman and J. C. Mitchell, "The British Headman in British Central Africa", *Africa,* vol. XIX (1949), pp. 89–106.

12. J. C. Mitchell, *The Kalela Dance,* Rhodes-Livingstone Paper No. 27 (1957); A. L. Epstein, *Politics in an Urban African Community* (Manchester: Manchester University Press, 1958).

13. W. Watson, *Tribal Cohesion in a Money Economy: A Study of the Mambwe People in Northern Rhodesia* (Manchester: Manchester University Press, 1958); J. van Velsen, "Labor Migration as a Positive Factor in the Continuity of Tonga Tribal Society," *Economic and Culture Change,* vol. 8, no. 3 (1960), pp. 265–278.

14. A. Kuper, *op. cit.,* p. 148.

15. For example V. Turner, *The Ritual Process: Structure and Anti-Structure* (London: Routledge and Keegan Paul, 1969), p. 6: "... the study of tribal ritual would certainly have been in the spirit of the institute's initial aspiration 'to study ... the problem of establishing permanent and satisfactory relations between natives and non-natives,' for 'satisfactory relations' depend on a deep mutual understanding."

> However, cutting across the differences of aims and methods of the two systems of government (i.e. the tribal and the colonial – v.T.), there are a few basic aims which they have in common. In many respects both groups, Administration and Tonga (both at home and abroad), work actively towards retaining what each considers the traditional tribal institutions. Furthermore, both groups are vitally interested in the maintenance of law and order, and in spite of the different premises from which they proceed, and the different ways whereby they want to achieve this aim (none of these matters are specified by van Velsen – v.T.), this common interest has been a very real incentive for reaching a compromise.[16]

Two extremes were implicitly depreciated: apartheid, the rigorous separation of races, and the overthrow of all native institutions in favour of radical western-oriented change.[17] In this opinion they did not differ from other British Africanists at the time. However, more important than a modest effort at implicit criticism, was the rigid separation between science, values and politics. The explicit moral concern, and the triumphant mood, of many conventional social studies was lacking. The general question of integration was something that was to some extent assumed; however, the focus on the questionable strength of the various systems signified the omission or suspension of value judgements. The strength/weakness of the system was opposed to the goodness/badness of the system. The opposition of both dimensions made a tragic attitude possible by widening the gap between the anthropologist and the people studied; in this way the anthropologist could evade the difficulties of reconciling his or her individual values with the colonial reality. Thus, while leaving no doubt about his aversion to *apartheid,* Gluckman argues:

> Nevertheless I insist that the facts we have to try to comprehend in our analyses show that over a fair run of time the Republic of South Africa is likely to manifest considerable internal stability (in the common-sense meaning of the word), and the sharp divisions between its colourgroups are unlikely to lead to revolution unless there is international intervention.[18]

Also:

> Portugal and Spain, where there are no effective divisions to disrupt determination to rule, though these countries as states are themselves very much

16. J. van Velsen, *op. cit.*, 1964. p. 317.
17. See M. Gluckman, *op. cit.*, 1975.
18. M. Gluckman, "Tribalism, Ruralism and Urbanism," in V. Turner (ed.), *Colonialism in Africa 1870–1960,* Vol. 3: *Profiles of Change: African Society and Colonial Rule* (London: Cambridge University Press, 1971), p. 127.

> weaker than the other European powers, are maintaining. their colonial positions. The Rhodesian and South African governments, who have only a small proportion of their enfranchised electors hostile to their polity, rule harshly but most effectively.[19]

Behind these predictions were the theoretical positions of the Manchester School (noting the additional factor of Gluckman's stress on the role of force). So Gluckman repeats his conviction that South Africa possesses some kind of cohesion as a result of the interdependency between institutions and persons:

> ... this interdependence within parts is communicated to the whole within which some overall cohesion also exists.

And he notes:

> the cross-linking of individuals within the total field in terms of a variety of associations and values which prevent most persons from becoming wholeheartedly loyal to one bond and hostile to all toher bonds....[20]

My point here is not the flagrant error of these predictions, as it turned out. I have already indicated the conservative bias of the theory. What is perhaps not so obvious is the tragic position which Gluckman sets out for himself; as an objective observer he had to make a diagnosis in spite of his own political wishes. While Gluckman at least is clear about his own preferences, other anthropologists became lone disappointed observers hiding behind the veil of professional discourse. Defending anthropological research during the colonial period, Turner has the following to say:

> It is true, of course, that in their personal capacity anthropologists, like everyone else, have a wide spectrum of political views. Some are known 'conservatives'; others lean far to the left. But as professionals, anthropologists are trained, over almost as many years as doctors (sic), to collect certain kinds of information as 'participant observers' which will enable them, whatever may be their personal views, to present as objectively as the current level of their discipline's development permits, a coherent picture of the sociocultural system they have elected to spend some years of their lives in studying, and of the kind of processes that go on in it. It is their ultimate duty to publish their findings and expose them, together

19. *Ibid.*, p. 150.
20. *Ibid.*, pp. 127–128.

> with an exact description of the means by which they were obtained, to the international public of their anthropological colleagues and beyond that to the "world of learning".[21]

The separation of science and values is here attained by the contrast between politics as a personal matter and anthropology as a common enterprise. Turner continues optimistically:

> Eventually, news of their work and analyses, through their own 'popular' writings or through citations, résumés (not infrequently bowdlerized) and digests by non-anthropologists, seeps through to the general reading public. Time thus winnows their reports and rids them of much that is biased and 'loaded'. There is no point in special pleading of tendentious argument; there are professional standards against which all reports are measured, and, in the end, the common sense of the common man.[22]

This conception of the professional role and its concurrent myths might be interpreted as a reassertion of the value or anthropological research in colonial Africa: ultimately, the anthropological insights will prove more potent than political pleading. Here, concerning the 'power' of anthropology itself, the mood is certainly not tragic. It is interesting to note that Turner feels a moral responsibility to the audiences of colleagues and 'the learned world', though not to the people studied; consequently, anthropology is shorn of direct political relevance, or at least, *emancipatory* political relevance.

A number of circumstances made the separation of science and politics desirable for the Manchester anthropologist. Of course, the professional had to confirm and reconfirm his image as a legitimate scientific disciplinarian, especially in view of the highly politicized context in which anthropology had developed, and most scholars sought an academic career in the western world. Another significant factor was the nature of the African fieldwork situation: the anthropologist was suspected both by the Africans and more often than not, by colonial administrators. The assumption of a strictly neutral role was, both psychologically and politically, an imperative of 'survival' in the field. Barnes, a former member of the Manchester circle in Africa, remarks in this context:

> Dependent on the goodwill of government acting as gatekeeper if not as sponsor, but equally dependent on the goodwill of the people they had come to study, anthropologists frequently found themselves subject to

21. V. Turner (ed.), *op. cit.*, 1971, pp. 1–2.
22. *Idem.*

> crossfire from which they could not easily escape by sitting on the fence. To mix metaphors further, we may say that they tended to bury their heads in the sand.[23]

Moreover, the content and methods of Manchester anthropology were not particularly relevant for developing an active political position on the issues of the day. Neither the analyses of petty politics, nor traditional usages, nor functionalist theory had contributed much to an understanding of the overall distribution or control of resources, or the dynamics of broader political struggles (Epstein is an exception; I will deal with his work below). Although irrelevant to transformative political practice, Manchester anthropology certainly was 'relevant' in a system sustaining sense. The focus on the *strength* of systems neatly coincided with the administrator's concern for efficiency, order and practicality.[24] Both the anthropologist and the colonial government shared basic assumptions: the emphasis on order, the concern for the viability of the colonial system, and the neglect of causes and dynamics of ultimate conflicts of interests and aims between the European and African populations. The immediate usefulness of anthropological insights and data[25] to the colonial Establishment was less important than the correspondence of their assumptions; it was that correspondence which moved government and private enterprise to finance research and publications in the (politically *and* scientifically) self-deceptive effort to evade the failure of the colonial enterprise.

I shall now turn to a treatment of Epstein's work on a Copperbelt town,[26] because it reveals the stresses within the Manchester paradigm when applied to a situation wherein Africans and Europeans consistently opposed each other. In his book Epstein analyzes the emergence of particular forms of African tradeunionism and nationalism, while isolating the town – itself a highly artificial colonial product – as his unit of inquiry. He presents detailed case studies of various confrontations between Africans and Europeans, and focuses on developments within the African community as a result of the requirements of the

23. J. Barnes, *The Ethics of Inquiry in Social Science* (Delhi: Oxford University Press, 1977), pp. 46–47.

24. M. Gluckman, *op. cit.*, 1975.

25. The usefulness of British anthropology at the time to colonial administration is not clear. Stauder is not particularly convincing on this point. See J. Stauder, "The 'Relevance' of Anthropology to Colonialism and Imperialism," *Radical Science Journal*, vol. 1 (1974), pp. 51–70.

26. A. L. Epstein, *op. cit.*

industrial system. Epstein assumes that the colonial system works; people can live together despite all conflicts and cleavages.

> Northern Rhodesia today is a society divided by many cleavages, of which the dominant cleavage is that between Europeans and Africans. This dominant cleavage affects very closely every aspect of the social system. Nevertheless, these cleavages do not follow simply the lines of colour. As we shall see, there are many divisions within the African community, while the White community is also divided within itself by various sectional and class interests. But underlying these cleavages, there is interdependence between all sections of the population, whose interaction make up the social system and keep it working.[27]

The main part of the book consists of a description of the successive clashes between the European administration and management leaders on the one hand, and the African working population on the other. Divisions within both populations are stressed, but these divisions diminish in importance when real economic issues are at stake. The clashes themselves are presented in such a way that the inequality of the antagonists and the role of force are clear: the gap between Europeans and Africans seems qualitatively different from the cleavages within both communities. The inconsistency of the book *is a result of the inadequacy of the theoretical framework in view of the very data presented.* Because of his assumption (the town comprises a system that works), Epstein is mainly interested in the mechanisms by which the system can contain all apparent conflicts. First he redefines, and refines, the rigid system notion:

> ...conflicting elements do not hinder the effective working of the urban system as a whole. They do not lead to a breakdown of the system because they operate within it. Thus I would suggest that the deeeper understanding of the urban social process in Africa requires a formulation in which inconsistency and disharmony are recognized not only as an integral part of the nascent social system, but also as an important source of its dynamic.[28]

He remarks that

> [the] major cleavages operating within an African Copperbelt community express conflicts of interest which can no longer be resolved in terms of a framework of norms and values commonly accepted as binding on the

27. *Ibid.*, pp. vii–x.
28. *Ibid.*, p. 227.

> community as a whole. Nevertheless, in its normative aspect, the urban social system also displays certain of the features characteristic of the tribal system.[29]

So he changes the focus from the conflicts of interests to the tribalistic features of the urban system: the last chapters of the book (Court and Community; Tribalism and the Urban Social System) concern the ways in which tribalism appears in the city. Here Epstein finds mechanisms by which conflicts are overcome. He elaborates especially on the principle of 'situational selection', as a means of reconciling conflicting norms. His interest in the normative integration of the urban system[30] thus leads him away from a dynamic perspective on the urban system (in the context of the overall colonial system) which demands to be understood as a result of the *ultimate divergence of interests between Africans and Europeans*. In his theoretical analyses Epstein is mainly interested in the Africans' capacity *to adapt themselves* to a developing system; he is not really concerned with the dynamics of the system itself. Of course the latter focus would lead him toward a consideration of the role of power differentials, the hidden or actual threat of force, etc. By neglecting these factors on a theoretical level, his study, despite its inconsistencies, reasserts the viability of the colonial system. It is political while disavowing practice.

Israeli anthropology

Although during the 1950s a few non-Israeli anthropologists individually undertook research in Israel (some of it of very high quality), on, for example, the kibbutz, the real establishment of anthropology as a discipline in Israel did not occur until the 1970s. The initiation in the mid-1960s of a large-scale anthropological research project, financed by the English Bernstein family and directed by Gluckman, was the immediate cause. Gluckman, himself a Jew and born of a prominent Zionist family in South Africa, had strong familial links with Israel. The so-called Bernstein Scheme gave him the unique opportunity to

29 *Ibid.*, p. 199.

30. The idealistic bias of Epstein's and Mitchell's studies is also stressed by B. Magubane, "A Critical Look at the Indices Used in the Study of Social Change in Colonial Africa," *Current Anthropology*, vol. 12, nos. 4–5 (1971), pp. 419–445.

combine his anthropological interests with his concern for the well-being of the country. With the assistance of E. Marx, who coordinated the research in Israel, he recruited and, in the professional sense, socialized several young anthropologists from America, England and Israel toward their doctoral fieldwork in Israel. All the studies dealt with the "Jewish" majority in Israeli society; Shokeid[31] remarks that Gluckman was interested in the question: 'What are the particular *Jewish* characteristics of various groups and institutions in Israel?' (emphasis by Shokeid).[32] Apart from the study of several *moshavim* (agricultural villages, to some extent cooperatively organized), the works cover kibbutzim, so-called development towns, and interaction patterns in urban settings.[33] Problems of bureaucracy are salient in almost all studies, while themes such as 'immigrant absorption' and the 'fusion of exiles' (important Zionist problems) appear several times as focal points in the analyses.

The pre-conceptions of Manchester anthropology are evident in the ways in which data are grouped and analyzed; in some studies, especially those of Aronoff, Shokeid and Deshen, this methodological approach is combined with typical forms of Manchester theory. As set forth in my presentation of the paradigm above, I regard this typical combination of an analysis of concrete events (implying a focus on rule and exceptions, manipulation of norms, etc.) and functionalist theory (concerning the ways in which conflicts contribute to integration, or generally, why they are not disruptive) are characteristic of the Manchester School. The most detailed illustration of Gluckman's theses is Aronoff's analysis of a development town in the Negev desert, which he calls Frontiertown (the choice of the name reflects Aronoff' conviction that the town is a 'pioneering' enterprise). The book deals with two main themes: the internal development of Frontiertown into a cohesive community and its external integration into the national political framework. The first theme is summed up by Aronoff as follows:

31. M. Shokeid, "Max Gluckman and the Establishment of Israeli Anthropology," in *In Memoriam for Professor Max Gluckman* (Jerusalem: Department of Sociology, Hebrew University, 1976), p. 11.

32. This is remarkable, because Gluckman's central argument in his chapter on "Anthropology and Apartheid" says: "...all men are alike in some respects and unlike in others, and all cultures are alike in some respects and unlike in others" (M. Gluckman, *op. cit.*, 1975., pp. 36–37).

33. See E. Marx, "Anthropological Studies in a Centralized State: Max Gluckman and the Israel Bernstein Research Project," *Jewish Journal of Sociology*, vol. 17 (1975).

> I argue that the merging of social and political leadership and activities in the early period of factional competition and strife was primarily responsible for the mobilization of large-scale involvement in public affairs. This involvement was related to the creation of social roles with which the participants identified themselves and one another as members of the same community for which they all cared and which they wanted to help develop. In this way the intensive factional strife led to cohesion at the higher community level.[34]

He follows Gluckman

> in trying to show how multiple cross-cutting conflicts of loyalty and allegiance where there is agreement on basic values, tend to inhibit the development of dichotomous cleavage, and in fact can contribute to greater communal cohesion.[35]

Moreover,

> [the] fact that the political structure was flexible in that it allowed for the full expression of disagreement with the ruling power, sometimes even to an extreme degree... made it a major contributory influence in the development of communal solidarity and spirit.[36]

Dealing with the second theme, Aronoff presents, among others, the following analysis:

> ... the right of the legally constituted and legitimate government of Israel to establish a new town and provide for its administration, at least initially, conflicted with the well-established democratic principle of responsible representative government, local as well as national... It is precisely such discrepancies in principles which allow the political actor to choose among them, to reject one authority as illegitimate at the local level while maintaining loyalty and belief in the legitimacy of authority at the higher level. In such a manner limited structural change can be initiated at the local level while maintaining and strengthening the national structures.[37]

So there is a Gluckmanian balance between conflicting principles at different levels, resulting in another success:

34. M. Aronoff, *Frontiertown: The Politics of Community Building in Israel* (Manhester: Manchester University Press, 1974), p. 2.
35. *Ibid.*, p. 12.
36. *Ibid.*, p. 13.
37. *Ibid.*, p. 108.

> ... the integration of the community into the institutions of national Israeli society ... (E)ven the limited local failures (i.e. the inability to maintain local autonomy and a high level of participation in public affairs – v.T.) were part of a greater national success.[38]

The book is an 'involved' and sympathetic account, full of vivid details of petty political strife and personal quarrels which, however, seem only to reinforce a basic commitment to the cultural enterprise of building up a new town. Certain circumstances heighten the dramatic content of the work. Aronoff's research was carried out during the initial stages of the founding of the town. The risks of overall failure, and disruptive strife, e.g. between immigrants from different countries were to some extent present; moreover, the town's inhabitants began a new life with all the personal insecurities entailed. Aronoff takes pains to assure the reader that the people were not primarily motivated by considerations of personal success or prestige, but joined the enterprise for primarily idealistic reasons. Even though the younger generation may not explicitly admit to idealistic motivations, this does not, according to Aronoff, necessarily mean that "[the son] is less idealistic than his father, but [it] merely reflects a different cultural attitude toward the expression of idealism".[39] Aronoff's intention here is to test the viability of the old Zionist tenet of pioneering for the country's sake *(chalutziut)* and his positive conclusion reinforces a conception of the town as a daring cultural enterprise in the Zionist tardition. A dramatic tension is generated because questions are raised about the viability or relevance of basic ideals.

This implies that the researcher is not a 'doctor' who presents a diagnosis of his case; on the contrary, Aronoff speaks about the failures and the successes of the town and Israeli society at large, and he is not backward in displaying and testing critically his own conception of what Israeli society should be. Aronoff's sense of participating in a sympathetic cultural undertaking is most obvious in his description of his 'unscientific' reasons for the choice of Frontiertown as an object of study:

> We [Aronoff and his wife] had been captivated by the rugged beauty of the barren Judeaen desert landscape in which the town was located. The appearance of the town itself was also important. It gave on the one hand the

38. *Ibid.*, p. 282.
39. *Ibid.*, p. 85.

> appearance of having been well planned and well maintained, yet it appeared to fit my image of what pioneering Israel should look like.[40]

The experience of sharing in a common enterprise was probably heightened by his decision to stay in the town during the Six-Day War in 1967. This apart, he succeeded – according to his own account – in creating a public image of himself as a politically 'neutral' person; he was allowed to participate in many gatherings of different political parties. In this way, I suppose, he made it possible for himself to be 'detached' on secondary issues of petty politics, while maintaining a basic commitment to the shared ideal of pioneering and self-help; thus he could avoid the sticky problems of too much involvement. The dramatic impression of Aronoff's book is further enhanced by the way in which he applies Manchester theory. The leading question is not (as in colonial Africa): How can the system contain the apparently deep conflicts and cleavages within it?, but: What is the raison d'étre of all conflicts in a presumably viable community such as Frontiertown? The second question is more concrete (Aronoff is not concerned with a system, but with Frontiertown) and testifies to a basic optimism: the viability of the community is simply assumed by all involved in its establishment. Aronoff thus structures his account in such a way as to prove the temporary triumph of Frontiertown and Israel at large by referring to the ultimate reconciliation of opposed elements, first on the intra-communtiy level and, second, between the local and national levels of Israeli society. Manchester theory provides the mechanisms by which these reconciliations are attained: cross-cutting links, structural flexibility, value consensus on socio-economic issues and basic authority patterns. The structure of the theory resembles an almost 'comedylike' mode of *emplotment*,[41] and thus contributes to the dramatic tension

40. *Ibid.*, p. 284.

41. For example, see H. White, *Metahistory: The Historical Imagination in Nineteenth Century Europe* (Baltimore/London: Johns Hopkins University Press, 1973), p. 9: "[Comedy is a special kind of story in which] hope is held out for the temporary triumph of man over his world by the prospect of occasional *reconciliations* of the forces at play in the social and natural worlds... The reconciliations which occur at the end of Comedy are reconciliations of men with men, of men with their world and their society; the condition of society is represented as being purer, saner and healthier as a result of the conflict among seemingly inalterable opposed elements in the world; these elements are revealed to be, in the long run, harmonizable with one another, unified, at one with themselves and the others" (emphasis in the original). The viewpoint of literary criticism might be a fruitful way to classify social theories; a number of striking parallels will appear.

apparent in the book. Having touched on some reasons for the theatrical impact of the work, I will now deal with the next question: just *what* is dramatically exposed? What does the triumph of Frontiertown and of Israel mean to Aronoff?

The succes of Frontiertown would appear to exemplify the vitality of western liberal democracy. Political pluralism 'within a basic consensus', in Frontiertown and Israeli society at large, reveals its integrative strength and adaptability *by the very presence of seeming weaknesses,* namely, fractional competition and strife, and the possibility to express disagreement with the authorities 'to an extreme degree'. Because of its stress on both flexibility and integration within a basic consensus, this type of Manchester theory is particularly suited to validate the presumed existence of liberal democracy in a bureaucratically organized society. This is further exemplified by the following analysis of Aronoff, in which he applies Coser's derivative (Marx via Simmel) notion of 'non-realistic' conflict,[42] hence the intracommunal political struggle

> could be considered to be a non-realistic 'war-game' where full political participation was originally denied by the nonrepresentative nature of the government administration... In this sense the local strife was a sound and fury signifying nothing – or at least very little. However, when one examines the positive contribution the local strife made toward the mobilization of the citizens in local public affairs and toward the development of a strong community spirit, it could be judged to be a classic example of successful non-realistic conflict!.[43]

The message here seems to be that limited political democracy on the local level is still 'functional' in spite of bureaucratic control from above. (In a sense, just because of this control, since the ultimate functional strife "was also an expression of the futility and frustration (at being unable] to control the course of the community's development".[44]) In this way, Frontiertown, according to Aronoff, epitomizes the possibility of a reconciliation between the various conflicting forces and principles in a liberal democratic 'welfare state', moreover, he is at pains to show

42. Coser retailed a similar theory about the integrative functions of social conflict. In non- realistic conflict, "the choice of the antagonists depends on the determinants not directly related to a contentious issue and is not oriented toward the attainment of specific results" (L. Coser, *The Functions of Social Conflict* (London: Routledge and Keegan Paul, 1956), p. 49; see also M. Aronoff, *op. cit.*, p. 110).

43. M. Aronoff, *op. cit.*, p. 110.

44. *Idem.*

that the Zionist notion of pioneering, while seemingly contradictory to the integrative principles of contemporary liberal democracy, is in a limited and 'controlled' form still a legitimate pursuit today; it contributes to the viability of the local community and, indirectly, of Israeli society. Aronoff concludes by claiming that Frontiertown recapitulates on a micro-scale, the general development of Israel from a pioneering society into a bureaucratically organized, but democratic state.[45] He suggests that Frontiertown stands for the viability of Israel at large.

In a somewhat less triumphant mood, but equally convinced of the ultimate viability of the communities they studied, both Shokeid and Deshen have extensively written on, respectively, a moshav in the Negev and a development town near Tel Aviv. Both anthropologists are 'veteran' Israelis and have engaged in relatively long and intensive fieldwork. Like Aronoff, Shokeid presents the people he studied not as mechanical role-performers, but as acting and striving within different social frameworks full of conflicts. In spite of many difficulties, the people of the moshav Romema (a pseudonym) – recent Moroccan immigrants from the Atlas Mountains – will ultimately 'adjust' in Israel. This assumption is implicit in the following sentence:

> It is difficult to predict what will be the *eventual* mode in which the Romemites will adjust in this sphere of behaviour (i.e., the political – v.T.) and how long its course of institutionalization will take.[46]

The reader receives a picture of people struggling on a predestined path. The drama is even heightened because, for quite a while, the Romemites choose the wrong path (in Shokeid's opinion):

> [during the period of Shokeid's later, intensive fieldwork] the strife among the Romemites appears largely to have been concerned with striving for unrealistic goals, that is, for a return to their past situation.[47]

However, the appointment of a political leader in the village, "whose qualities and emotional empathy had an appeal beyond the borders of political commitments",[48] is enough reason for Shokeid to regard Ro-

45. *Ibid.*, pp. 281–282.
46. M. Shokeid, *The Dual Heritage: Immigrants from the Atlas Mountains in an Israeli Village* (Manchester: Manchester University Press, 1971), p. 212, emphasis added.
47. *Idem.*
48. *Idem.*

mema's case as representing "a movement towards *'radical structural change'* ".[49] Typically, the seemingly disruptive strife contributes to the Romemites' ultimate adjustment:

> the group begins to establish a completely new system of relationships in spite of, and in some ways because of, its continuing strong attachment to the values and goals, and the patterns of strife, of its earlier situation.[50]

And in the conclusion of the book, all doubt about Romema's viability disappears:

> The Romemites through the trials of internal strife and the efforts of coping with a strange environment have emerged as a viable community of successfull farmers.[51]

A somewhat similar analysis can be found in an earlier work by Shokeid (concerning another immigrant moshav) in which he focuses on the positive functions of factional rivalry at a particular stage of adaptation:

> The economic developments and the accumulation of possessions, which were to a large degree supported by the rivalry between the kin groups, brought about new interests and interest groups which cut across the kin groups. These conflicting sub-systems, criteria and principles brought about the disintegration of the groups whose consolidation upon traditional elements might handicap in the long run rationalization and adjustment to more modern and dynamic conditions. And thus the emergence of factions which provided in the first stage some loyalties and power for co-operative activities to deal with the new environmental and social conditions, at the same time prevented a development of rigidly formed groups which could impede change and adjustment in the later stages.[52]

In other words: originally the factions were functional to the immigrants's adaptation; in the later stages they would be dysfunctional, but, as a result of cross-cutting ties, they disintegrated. One encounters here a form of teleological reasoning: the disintegration of the factions is explained by referring to the later (apparently inevitable) 'rationalization

49. *Idem.*, emphasis in the original.
50. *Idem.*
51. *Ibid.*, p. 231.
52. M. Shokeid, "Immigration and Factionalism: An Analysis of Factions in Rural Israeli Communities of Immigrants," *British Journal of Sociology*, vol. 19 (1968), p. 404.

and adjustment to more modern and dynamic conditions.' As in Aronoff's case, the question might be asked: what pre-conceptions lay behind the dramatic Manchesterian exposition of the Romemites' struggle for a better life? To get a better grip on this problem, it is necessary to clarify briefly the position of "Oriental" Jews vis-à-vis Western Jews in Israel. Many Western Jews – who dominate the positions of economic and political power in Israel – display deprecatory attitudes towards Oriental Jews, especially those of Moroccan and Kurdish origin. This attitude is reinforced by tales and rumors of various kinds, for example, with reference to the 'Arab-like' sectarian quarrels in certain Oriental Jewish communities, the aggressive stance towards outsiders, and their habitual backwardness. As elsewhere this gossip easily slips into racism. Many Oriental Jews have become disappointed with their lives in Israel because of discriminatory treatment and their low status (e.g., the Moroccan Jews in Jerusalem, who formed the Black Panther group). However, during the period of Shokeid's fieldwork many Oriental Jews still hoped for acceptance in western-oriented Israeli society. Shokeid studied the Romemites with the purpose of writing a book for a western audience (partly an Israeli one). As a western 'veteran' Israeli, he participated in while observing for some time, the lives of recent Oriental Jewish immigrants; this rendezvous between anthropologist and object activated the basic ideological concern for 'the fusion of exiles' (i.e. immigrants from various countries), at least among the Romemites. According to Shokeid:

> To the Romemites, my association with them, especially because of our close relationships, epitomised, to some extent the aspirations toward integration of the returning exiles. They insisted that I partake as an equal of their food, particularly of their drinks, during Sabbath meals and on other occasions. This bestowal of equality to them became symbolic of the bridge that could span the social distance between us.[53]

It appears that the fusion of exiles embodied (for the Romemites) two related ideals: on the one hand, the experience of a common national and cultural identity, and on the other hand, the bridging of the status gap between Westerners and Orientals in Israel. Their stress on equality in an intimate setting expressed their concern for both aspects of the

53. M. Shokeid, in S. Deshen and M. Shokeid, *The Predicament of Homecoming: Cultural and Social Life of North African Immigrants in Israel* (Ithaca, N. Y.: Cornell University Press, 1974), p. 55.

same ideal. Undoubtedly, however, their attitude to Shokeid had ambiguous aspects. Since he was a representative of a high status group, it was attractive for the Romemites to ally themselves with him: on the other hand, he must have been perceived with an undercurrent of antagonism, especially when he failed to conduct himself as a 'real' Jew. Shokeid thus writes about the possibility that the Romemites could be 'deeply offended' by his records:

> In their struggle for social esteem among the different sectors of Israeli society, the Romemites might blame my account for stripping and humiliating them in public. I could also be considered as representing the hypocrisy and seemingly high-handed manner of the veteran Ashkenazi (i.e. Western – v.T.) Israeli stratum.[54]

The anthropologist, then, became a medium through which the Romemites – recent immigrants and socially rather isolated in the Negev – exposed themselves to the broader (western) society. To reassure them of "his deep sympathy and respect",[55] and thus to enlarge his own sense of freedom in his work, Shokeid decided to adopt a new Hebrew surname (his earlier name was Minkovitz), associated with the name of the village. It is easy to conclude that Shokeid here consummated an unspoken transaction: in his own way, he responded to the Romemites' expectations about a fusion of exiles by reaffirming their common root. More generally, Shokeid 'responded' to the Romemites, in the social context of their joint cultural enterprise, by humanizing and equalizing his relation with the Romemites in the anthropological accounts. As indicated, he presents a basically sympathetic and 'committed' picture of the Romemites and reasserts the supposed viability of the community. However, the character of the fieldwork situation (within the context of general Western–Oriental relations in Israel) has influenced Shokeid's analysis of factions: the vivid presentation of so many quarrels and manipulations without asserting the ultimate viability of Romema could indeed be regarded by the Romemites as an implicit insult, and an affirmation of the stereotypes (the 'backward' nature) of Oriental Jews. Theoretically, Manchester anthropology is well suited, even pre-adapted, to express what Shokeid had to say: these people were struggling and striving on their way to success. Their apparent failings, i.e. their un-

54. *Ibid.*, pp. 57–58.
55. *Ibid.*, p. 58.

realistic strife and adherence to their former way of life could be interpreted sociologically as no failure at all, but as an ingenious adaptation to the 'modern' and 'dynamic' conditions of Israel.

I return to the original question: what dramatic message did Shokeid feel compelled to offer to his western audience? The answer seems clear: Shokeid's book can be read as a demonstration of the capacity of present-day Israeli society to absorb its highly diverse immigrants. It documents a crucial test of the ways in which western Zionist ideals work out vis-à-vis Oriental Jews. The triumph of Romema thus becomes the triumph of the existing order; Manchester theory here helps to present the struggles of the people on their path to the 'modern' status quo, under the earnest stage direction of the anthropologist. (Interestingly, Shokeid's argument differs sharply from the opinion of another Israeli anthropologist, the Arab Kanaana, who believes that the transformation of Israeli culture into a Middle Eastern culture is "the best possibility for a solution to the Middle East situation".[56]

Deshen was even more 'involved' than Shokeid in his focus on the Israeli development town 'Ayara'. For several years prior to his research engagement he had lived in Ayara,[57] and through earlier professional contact, had developed an affinity for the traditional Djerban Jews from Tunisia, a group with which he was to be especially concerned in his study. Moreover, he played an active role in a religious party during the local election campaign that turned out to be the analytical focus of the book. He is explicit about his own preferences:

> I am an Israeli and I accept the basic ideological premises of Israeli State and society ... Since I am personally attached to traditional Judaism, I felt an affiinity with traditionalminded immigrants (particularly from North-African countries).[58]

In his introduction Deshen describes how 'basic ideological premises' were fulfilled in Israel:

> Immigration brought together Jews from various backgrounds, from countries like Yemen, Morocco and India, from Eastern Europe and from countries of the West. They spoke many different tongues and each worshipped

56. S. Kanaana, *Sociocultural and Psychological Adjustment of the Arab Minority to Life in Israel* (California: R. and I. Research Institute, 1976), p. 144.

57. S. Deshen, *Immigrant Voters in Israel: Parties and Congregations in a Local Election Campaign* (Manchester: Manchester University Press, 1970), p. 58.

58. *Ibid.*, p. 8.

in his own tradition. Yet they shared Jewish ancestry and culture: their one common language was liturgical Hebrew, with which most were familiar to at least a small extent. This conglomeration has forged itself into a proud and selfconscious nation.[59]

Like Shokeid, Deshen is basically concerned with the absorption of immigrants, but because the locale he studied was an ethnically heterogeneous city, he focused less on the problems of factional strife in the process of immigrant adaptation than on expressions of political ethnicity. As suggested in my treatment of Shokeid's fieldwork, political ethnicity is generally regarded in Israel as a highly sensitive issue, because it contradicts the Zionist ideal of the fusion of exiles. But Deshen considers this factional phenomenon as a necessary stage in the process of the immigrants' political adjustment:

Viewed in more abstract and general terms the data lead to a point of theoretical importance in considering the process of absorption of immigrants. According to a recent formulation, "the absorption of individuals is expressed by the extension and specification of their relations" [Deshen refers here to a paper by E. Marx – "Theoretical Aspects of the Absorption of Immigrants," Department of Social Anthropology, University of Manchester, 1964 – v.T.]. In the Israeli immigrant situation this would imply that the formation of social and political allegiances and ties on ethnic grounds marks a certain stage in the process of acclimatization of the immigrant to his new social surroundings. This seems to have been the case in Ayara in 1959, particularly in respect of the Moroccan immigrants.[60]

Deshen then documents the extension of various ties beyond the borders of the ethnic group. In another place, he sums up the reasons for the decline of political ethnicity in Ayara. He mentions, first, the emergence of new groups that cut across existing ethnic lines (such as those based on religio-cultural and socio-economic interests) and, second, the influence of the Six-Day War which, he estimates, has had a lasting impact on the community.[61]

The analysis of political ethnicity as a stage in the process of adaptation is apparently based on Epstein's and Mitchell's analyses of tribalism in Copperbelt cites. Deshen informs us:

59. *Ibid.*, p. 1.
60. *Ibid.*, p. 78.
61. S. Deshen, " 'The Business of Ethnicity Is Finished!' ? The Ethnic Factor in a Local Election Campaign," in A. Arian (ed.), *The Elections in Israel – 1969* (Jerusalem: Academic Press, 1972), p. 299.

> the thesis is that social institutions which have seemingly traditional characteristics emerge under conditions of drastic change to fill new needs. They are in fact very different from the old institutions which they seem to resemble.[62]

This notion is identical with Epstein's and Mitchell's argument that tribalism persisted in different forms in the Copperbelt towns because it was functional in the Africans' adaptation to the requirements of the urban social system. Apart from being a functional stage in the process of adaptation, some expressions of ethnicity can, according to Deshen, also be considered as reflecting the many ambiguities in human relationships:

> Israeli ethnicity might be seen in terms of an idiom that covers a broad spectrum of problems rooted in the particular complexities of human interaction in the Israeli situation. This view of the social function of some of the beliefs that people hold has been developed and richly documented by students of African witchcraft beliefs and accusations.[63]

The expression of ambiguities in various customs – witchcraft, and one may add, ritual conventions – was a theme that was adopted and used in a particular way by the Manchester School in Africa, especially, by Gluckman, Turner and Marwick. According to Gluckman, the contained expression of conflict in these customs contributed to the cohesion of the group (see above).

But one must immediately observe, since this is so basic to the approach involved, that apart from the fact that the Manchester notion does not exhaust the function, or better, meaning of ritualized ambivalence, it is impermissible to transform the *existential* contradictions, which are an aspect of primitive ritual, into the socioeconomic contradictions of contemporary Western society, or to the related circumstance of Colonial conquest of pre-capitalist peoples. The effort to do so is a typical category mistake, witting or unwitting, which assimilates *all* contradictions to a common definition and to a common function. There is no recognition, for example, of the nature of *non-antagonistic* contradictions which may occur in certain societies, and there is certainly a denial of the reality of intractable socio-economic and cultural contradictions in, for example, contemporary capitalist society that have a *revolutionary* potential. The latter cannot be resolved by their signification in ritualized

62. *Ibid.*, p. 298.
63. S. Deshen, *op. cit.*, 1970, p. 217.

acting out. In a word, Manchester anthroplogy had deformed and diminished the notion of socioeconomic contradiction, which has its roots in the Marxist tradition (it should be recalled that Gluckman is considered to have a radical background), and turned it into its opposite, namely, a system sustaining, homeostatic process. The claim that socio-economic contradictions are, so to speak, aboriginally apparent and solvable in the absence of change serves, of course, to rationalize all contradictions in the anthropologist's own conflicted and alienated culture. There is perhaps no clearer example in the anthropological literature of the way in which perspectives on traditional societies reflect – even when evading – the realities of one's own time. So far as the problem of *existential* ambiguity is concerned, it bears no relationship whatever to the social contradictions on which the Manchester school has built its case, but that would take us too far afield – except that it is necessary to re-emphasize here that Gluckman and his followers have implied that the over-generalized conflicts which they presumably document have their roots in human nature; thus they indiscriminately naturalize a social process, depriving it of its history and particularity, in a typical structuralist mode.[64]

Deshen thus reflects the Manchester convention when he speaks of 'the social function' of these beliefs (in the Israeli context expressions of political ethnicity), implying that they are not just 'superfluous', but fulfill a need. The conclusion is that forms of political ethnicity serve different functions in the process of the immigrants' adaptation, and thus do not disrupt the Israeli order.

The process itself is sketched as a gradual one: a radical breach with the past is non-functional and moreover, if desired, signifies a wrong attitude toward the cultural roots of the Orientals. Deshen thus pleads for modernization in tandem with the preservation of previous customs, e.g. religious conventions, as far as these do not hinder the community's development in the economic and social spheres.

The book ends with a description of elections in Ayara: "People went to vote; and thus they marked their emancipation and sovereignty".[65]

64. I am grateful to Stanley Diamond for clarifying here the dynamics and dialectics of contradiction and conflict. For a more extented discussion of the perspective here put forward, see S. Diamond, *In Search of the Primitive: A Critique of Civilization* (New Brunswick, N. J.: Transaction Books – E. P. Dutton, 1974), especially chapters 1 and 4.

65. S Deshen, *op. cit.*, 1970,. p. 219.

"The acrimony and pettiness of the past weeks seemed to have gone. There was a sensation of straightening of shoulders and raising of heads."[66] Deshen structures his account in the form of a 'success story': a detailed analysis of all types of manipulation and allegation is followed by an impression of the ultimate overall political equilibrium. It convinces the reader that the strife was secondary to the broader unity of the people. Deshen's work on Israeli society in general is directed by the same question as the one with wich he dealt in his study of Ayara:

> ... one of the most intriguing sets of problems that confront the student of Israeli society [is]: how do sentiments of national and civic identity under conditions of great social heterogeneity actually emerge? And, in particular, how is such a potentially disruptive element as ethnicity tamed in the heterogeneous Israeli sociely?[67]

The author's commitment is clear, and the formulation is revealing: he assumes that political ethnicity indeed can be tamed in Israeli society; the question he thus poses is *how* it is tamed. Deshen's answer to his own questions is not surprising:

> The data suggest that manifestations of ethnicity in the forms of ethnic sentiments and identity are stimulated in such a way that they are subservient to general Israeli civic and national loyalties. Ethnicity is legitimized in terms of 'unity in diversity' only; it does not figure as a legitimate end in itself.[68]

This generalization apparently refers to ethnicity in Israeli society at large; interestingly it is *only* based on observations of the local election campaign in Ayara and on the memorial celebrations of Tunisian immigrants. In conclusion, Deshen provides us with two interpretations of the function of political ethnicity: first, it serves the immigrants' adaptation and second, it does not disrupt the Israeli order. The viability of immigrants' groups does not contradict the general integration of Israeli society – a judgement which is almost identical with Aronoff's conclusion concerning the relation of Frontiertown and Israeli society at large. In both cases the application of Manchester theory with its

66. *Idem.*

67. S. Deshen, "Political Ethnicity and Cultural Ethnicity in Israel during the 1960's," in A. Cohen (ed.), *Urban Ethnicity,* ASA Monograph No. 12 (London: Tavistock, 1974), pp. 302–303.

68. *Idem.*

stress on the ultimately unifying function of all contradictions, is notable. Aronoff, Shokeid and Deshen combine a 'committed' picture of the subjects of study with various forms of functionalist Manchester theory. It is easy to find further examples of this theory in Israeli anthropological works,[69] e.g. on the ways in which conflicts between Israeli bureaucracy and settler communities contribute to the viability of these communities or their integration into the larger society. However, the interesting fact remains that the three authors mentioned are, as far as I know, the most concerned of all Israeli anthropologists, with the viability of both the communities they studied and Israeli society in general; at the same time their works evidence the 'purest' examples of Manchester theory. Their overall commitment to the viability of Israeli society (its bureaucratic integration of different groups and its ideological integration by the reconciliation of various Zionist principles) fits, then, the structural functionalist notion of the existence of system integration, despite, and partly because of, apparent cleavages and conflicts in parts of it.

Conclusion

There is a fundamental continuity in the problems posed by the Manchester School in Africa and Israel: in brief, what is the actual strength of the dominant systems, respectively colonial Indirect Rule and post-statehood Zionism? Analyses of conflicts and cleavages (between White and Black, Westerners and Orientals) began either from the problem: 'how might conflicts contribute to broader integration, adaptation and adjustment', or: 'why are these conflicts and cleavages not disruptive to the general order'.

Whereas in the studies on Central Africa the strengths/weaknesses of the system were relatively opposed to the goodness/badness of the system, this is not the case in the studies of Israeli communities. These latter project a critical assessment of the viability and relevance of old ideals applied to new groups (e.g. Oriental Jews) under new circumstances (e.g. a high level of bureaucratization). Here the anthropologists implic-

69. See T. van Teeffelen, *Anthropologists on Israel: A Case Study in the Sociology of Knowledge*, Papers on European and Mediterranean Societies No. 9 (Amsterdam: Anthropologisch-Sociologisch Centrum, University of Amsterdam, 1977), chapter IV.

itly test both the strength *and* the 'goodness' (i.e. relevance) of Zionism as embodied in Israeli Jewish society. In fact, the works on Israel do not only elaborate on the principles that lie behind the supposed integration of the society (as in the studies on Africa), but also on the principles that might reconcile reality and ideology. Since values are explicitly introduced into the analyses, the forms of presentation, and generally the mode of discourse are changed: the distance between the anthropologist and his objects is minimized by the use of a literary style, sympathetic words, the introduction of the anthropologist himself into the account, the presentation of people as subjects who make their own history.

Manchester forms of theory and presentation are not only deployed as explanatory devices, but may also be used to heighten the drama of the story itself: petty conflicts ('human weaknesses') contrast with devotion to common ideals; minor social cleavages only accentuate broader cohesion. Moreover, people are not presented as role-performers, but as human beings striving and struggling toward a better life – within the broader status quo. This mode of discourse might be partly explained by referring to the fieldwork situation in which these Israeli anthropologists were involved. All authors shared to some extent in a common cultural enterprise with the people they studied. Aronoff felt himself a member of a pioneering community: Shokeid had to meet expectations concerning the 'fusion of exiles,' while Deshen was involved in an election campaign. However, these anthropologists were not only committed to the people they studied, but also, out of their basically Zionist orientation, to the larger integration of Israeli society on western lines. Their analyses suggest ways in which these commitments can be reconciled on a theoretical level.

As stated in my review of the works of the Manchester School on Africa, the fieldwork situation there was characterized firstly by some measure of suspicion from both the colonial administration and the Africans, and secondly by the psychological and political difficulties encountered by the anthropologists in their efforts to identify wholeheartedly either with the colonial system or with the Africans as a political group. The anthropologist thus became an observer who identified with academe or with individual persons during fieldwork. The distance between the anthropologist and the Africans was reflected in the analyses by the rather strict separation between science and values: the strength of the system was studied, not the viability of either the European or African communities. The anthropologist took plains to look 'neutral'

to his/her western academic audience,[70] by means of his academic interests and formulations.

However, these differences in modes of discourse are subordinate to the real continuities in the applications of the paradigm. Functionalist Manchester anthropology, as applied both in Africa and Israel, tests the strength of the dominant systems within the status quo. Far from being a critical test, the theory itself presupposes the answer by the choice of assumptions and concepts.

70. Which words, formulations, style, etc. are regarded as "neutral" depends on the social characteristics of the audience.

17 Latin America: The anthropology of conquest[1]

JÜRGEN GOLTE

Manas imatapas yachaniñachu, atrasus kayky;
huk umawansi umaykuta kutichinqaku.
Manas sonqoykupas allinchu; ancha mancharis-
qas, nisiu weqeyuqsi, waqaq yuyapa hina, na-
kasqa turupa hinas; chaysi mana allinchu.
Huk ducturkunas chayta nin; kikin allpanchikpi
miraq, wirayaq, qilluyaq ducturkuna.
Nichkachunku ya, hinata nichkallachunku.[2]

J. M. Arguedas

Anthropology has a long history in Latin America. Wherever the colonial system encountered a large native population which could be easily exploited and integrated, social research, in the name of science, flourished. The objective of that 'scientific' concern has been clear, namely an ordered knowledge of the social, economic, and ideological for-

1. Translated from the Spanish original by Thomas Moore.

2. "They say we don't know anything now, that we are backward, that we must change our heads for better ones."

"They say our hearts are also unsuited to the times full of fear and tears, like the heart of the lark or the heart of a great beheaded bull, and therefore insolent."

"They say some learned men assert this of us, doctors who proliferate in our land, here fatten themselves or grow pale."

"Let them go on talking and chattering if they like."

(From the poem "Huk Doctorkunaman Qayay", Invocation to the Doctors (Arguedas 1972, pp. 49–52), written in 1966 to express his differences with the work of the Instituto de Estudios Peruanos, the most important social research institution in Peru during the decade of the 1960's.

mations, useful for the restructuring of the native societies for the benefit of both the immediate conquering group and the metropolitan country. In addition to these scientific integrationists, others have sought integration on the level of the developed ethic of the metropolitan countries, which has been critical of the economistic and destructive forms of integration. Moreover, it must be noted, the conquered people themselves, in their opposition to the prevailing mode of subjugation, have attempted to reveal the genius of the autocthonous system to the conquerors.

These three tendencies were represented in the decades following the Conquest. The first two have prevailed ever since in varying degrees of intensity, while the native response has faded as their cultures have been forced into decline.

Today the social sciences are commonly accused of identification with imperialist and dominant class interests, but the possibility of a liberating, anti-colonialist, and anti-imperialist anthropology is suggested. An analysis of Latin American anthropology would be meaningless unless this problem is kept in mind. The history of Latin American anthropology must be considered in terms of its relationship to the history of those cultures and classes which imperialist anthropology treats as dominated objects in every sense of the term, and which a liberating anthropology seeks to liberate.

In our analysis of this problem, we shall focus on two salient regions, the Andes and Mesoamerica. The Inca state, into which the pre-Hispanic societies of the Andean region immediately prior to the Conquest were organized, was composed of a number of ethnic groups which were largely self-sufficient economically, maintaining relative political autonomy through this process of social reproduction. It was a class society in which functional elites had emerged on the basis of: (1) the construction and maintenance of extensive irrigation works on the arid coastal strip, (2) the administration of a system of exchange of products among peoples at different ecological levels of the Andean slopes, and (3) the domination and domestication of nature in both regions. Moreover, the military power of the Inca group led to their dominance within the state. The peasants and artisans, or *hatunruna*, provided labor for the administrative elite in the areas assigned to the latter and in public works. The privileged elite, in turn, redistributed a portion of the products of this work to the *hatunruna*. A servile population, the *yanacona*, worked full-time for the elite in a variety of crafts and other tasks and was sustained from the reserves of the state (Murra, 1956, and Golte, 1973).

This society was conquered and dismantled by the sixteenth century Spanish who sought to acquire personal wealth and to extract precious metals for the metropolitan country. After a short period of outright plunder the conquerors tried to restructure the society for their own benefit.

This restructuring began with general surveys (e.g. Diez de San Miguel, 1964) of the economic potential of the populations—the number available for work, amount of arable land, existence of mineral deposits, social and economic organization of the region, system of tribute, and amount of labor service provided the ethnic group leaders for the use of the Inca state.

A parallel effort to understand and systematically analyze the pre-Hispanic social and economic organization arose, not from the original conquerors, who wrote accounts and impressions of the conquest itself, but from men like *licenciado* Juan Polo de Ondegardo, with backgrounds in Spanish universities. Polo came to Peru to fill a number of positions in the colonial administration, such as *corregidor* in Cuzco. Working with informants from the elite of Cuzco and from the provincial population of the southern Andean region, his systematic analyses and descriptions of the economic organization of the Inca state were designed for use in the implementation of the reorganization of the society to suit colonial interests with a minimum of conflict. He also studied the ideological system, aware that understanding it was a precondition to the economic and social transformation (Polo de Ondegardo, 1916a, 1916b, 1916c, 1917, 1940). Moreover, his descriptions of Quechua religion were clearly intended to prepare the basis for the implantation of Catholicism. In brief, Polo's works were written for the purpose of transforming the society analyzed: this society was regarded as an object to be changed, an object to be manipulated by the social group to which the analyst belonged.

From the opposite perspective, the native population interpreted the process of subjugation in terms of its own worldview. The Christian God defeated the Andean gods in one more cataclysm in a history perceived as cyclical and determined by the action of supernatural forces. The native response, therefore, necessarily assumed the form of messianic movements whose leaders were possessed by the native gods and prophesied their return (Wachtel, 1971; Duviols, 1971, pp. 107–112; Millones, 1973a, 1973b). This reaction led to a massive Spanish effort to understand and

analyze the native ideological system for the purpose of destroying it.[3]

A second type of response to the reformation of native society was that of those natives who were educated by the colonial Spanish. The most outstanding of these were Garcilaso de la Vega and Guaman Poma de Ayala, both descendants of the pre-Hispanic elite.

Garcilaso went to Spain at age twenty and as an old man wrote his *Comentarios Reales* (1960), in which he reinterprets Inca history and society in terms of the Renaissance humanist and neoplatonic utopia, Despite his efforts to bring together the European and Inca universes into one harmonious system, his work became a protest against colonial politics on the basis of the fundamental inequality of Europeans and natives the latter being considered objects of exploitation (Garcilaso 194, 1962).

Poma de Ayala wrote his *Nueva Coronica y Buen Gobierno* with the purpose of making Andean society acceptable to the Europeans (especially to the king of Spain) as equal to their own. He devised a common fictitious history for both societies. Nevertheless, he describes in detail the exploitation and abuses of the conquerors. The utopia he describes is not the Renaissance utopia but the Inca state. The entire chronicle is an attempt to convince the just and good Spanish king that his subjects are altering the just and good Inca society without the king's authorization (Poma de Ayala, 1936).

In Guaman Poma we have a prime example of social science committed to the oppressed society. But we must not forget that the motivation of the native writers was largely personal affirmation. As descendants of kings and of provincial nobility, they were trying to regain a privileged position. It is at the moment of the dismantling of the autocthonous society that the conquered tried to make the conquerors understand their point of view, in part by adopting the conquerors' methodology. This was not repeated with the same quality, precision, and comprehension in the centuries that followed.

The integration of the native population into the colonial system was implemented by the conquerors' social science. The Andean social and

3. For data on these research efforts and the application of the knowledge thus acquired in the campaigns for the "extirpation of idolatry," see Duviols, (1971). The most accomplished works of the period are Arriaga (1920) and Avila (1967). The vocabularies and grammars of the aboriginal languages, elaborated in the second half of the sixteenth century and the first third of the seventeenth century must also be considered a part of this response.

economic system was radically transformed. The native administrative hierarchy was destroyed, and the Inca bureaucracy was eliminated, except where it was integrated into the colonial group through marriage ties with the conquerors' families or through other forms of participation in the Spanish system. The political power of the ethnic group leaders was taken over by the conquerors, who required them to collect tribute for the Spanish. The native peasants were grouped into *reducciones*, reservations in which the annual election of authorities and the common ownership of the means of production were established. The inequality and dependence of colonial society were expressed in the ties with the Spanisch – tribute in kind and forced labor in the mines.

The regional organization was controlled by the Spanish through the *encomienda* system, in which a Spanish *encomendero* was placed in tutelage over a native group from which he had rights to extract tribute. He, in turn, was responsible to the Spanish Crown. The unsatisfied *encomenderos* sought to make their position hereditary but were opposed by the Crown; so, new forms of private dependency arose between the native population and the Spanish. The most important of these was the *latifundio*, a large hacienda, or landed estate, on which the Spaniard established himself as owner and the native population remained as labor force, receiving as compensation for its work the right to use some of the lands for its own sustenance. Some communities of natives were left outside the *latifundios* and were forced to work in the mines and collectively pay tribute to the colonial authorities.

The integration of the native population proved so effective that there were few attempts at organized insurrection during the colonial period. And since there was no longer a need for information about the situation of the exploited peoples after the period of dismantling and reordering of native society, social science declined. Only in the eighteenth century, when nascent British mercantilism threatened Spain's overseas domain, and the natives, overexploited through forced trade, rebelled, do new social scientists appear on the scene.

The best known of these were Jorge Juan and Antonio de Ulloa, children of the Enlightenment, who travelled through the Spanish colonies in South America on a mission for the king of Spain. The result of their research was an objective analysis of social and economic conditions in the colonies. They easily discerned and described the system of exploitation of that time but did not take sides. They wrote from a detached, superior position, relative to the peoples described. Their *Noticias*

Secretas de América (1953), written as a secret report to the Spanish Crown, was useful for a variety of purposes. It fell into the hands of the British, who were eager to denounce the pre-mercantile oppression of the American native population in order to promote their own mercantile ventures; so, they published the analysis.

Significantly, in the nineteenth century there were abundant reports of travellers referring to natural conditions, especially mining potential, but there was no social science in Peru. The political independence of the country in 1821 did not basically modify the dependence of the native population. The number of *latifundios* increased at the expense of the native communities, but lawyers and judges proved more valuable in that conquest that anthropologists.

Meanwhile, the organization of the native communities began to disintegrate as a result of individualization in their relations with the outside. During the colonial period, these relations had been collective systems of forced labor and tribute, and in the eighteenth century even forced trade. But in the republican period tribute was collected by households, and, above all, commerce developed on an individual basis.

Commercial penetration came about because the traders from the capital wanted both a market for their manufactured goods in the provinces and agricultural products to trade in the growing urban market. The haciendas, which formerly produced food supplies for consumption in the cities, began cultivating cotton and sugar cane for export to satisfy the growing metropolitan demand.

Under these conditions, members of the native communities, motivated by personal profit from commercial relationships, spontaneously decided in communal assemblies to distribute communal lands as family properties. Thus, the native communities came to have internal differentiation in their land holdings, which led to the formation of a rural proletariat on the one hand and a class of rich landowning peasants on the other. This process reached its maximum development in the 1930's.

Prior to that time, however, a new anthropology emerged in Peru. Its primary concern was to turn the natives into seller and consumers, an urgent task of the commercial bourgeoisie, especially with respect to the Quechua population, which had remained outside the commercial circuit in the hacienda system. After the 1879 defeat in the nitrate war with Chile, the Peruvian urban bourgeoisie suddenly became aware that the country was not progressing toward modernity in accordance with liberal theory. They sought the source of this lack of progress in the native

sector, which for centuries had supported the bourgeoisie with its labor. The 'backward' natives, especially in the hacienda system, as opposed to the 'modern' bourgeoisie, were seen as the underlying factor in this 'backwardness'. The urban population became interested in the 'modernization' of the native and in his inclusion in the commercial circuit. It began to concern itself with the education and 'civilization' of the peasants.

But the bourgeois intellectuals allied with commercial capital, were limited in their desire to convert the largely self-sufficient natives into buyers by the highland landowners. The latter consciously opposed the breakdown of the Quechua tradition among the servile population, which they rigidly controlled as the basis for maintaining the hacienda system. By controlling the culture they lessened the possibility that the natives might perceive alternatives to their servile condition. For this reason the landowners, as well as the servile natives, became a target of the intellectual bourgeoisie.

The landowner was credited with the 'degeneration' of the native and denounced as a relic of colonial society. The bourgeois intellectuals idealized the precolonial Inca state as prefeudal and rediscovered its Renaissance image. For them the social and economic order of the Inca state was a utopia and its dominant class philanthropic philosophers. The native community, with its internal democratic order, the colonial origin of which was completely denied, was for them both a vestige of the Inca state and a nucleus for a new society.

All this brought about the rise of Peruvian *indigenismo* among the bourgeois intellectuals. Unlike Mexican *indigenismo*, it was primarily antifeudal and developed a culturalist anthropology which largely collected data on folklore and customs. There was little need for application, as in the early years of the colonial period; so, it became impressionistic and almost totally lacking in theory. Nevertheless, *indigenismo* was effective in inspiring legislation to protect the native from expansion of the *latifundios*. This legislation tried to preserve the communalistic character of native villages at a time when the disintegration of the communities was already very advanced, and when the most fertile lands of the communities were already in private hands.

The concept of native communities of pre-Hispanic origin and haciendas of colonial origin is the basis for the Marxist interpretation of J. C. Mariategui (1964). Through his essays the ideas of *indigenismo* have influenced the interpretation of Peruvian peasant history and culture to this day.

Moreover, the re-evaluation of pre-Hispanic societies brought about by *indigenismo* led to the development of pre-Columbian Peruvian archaeology with Julio C. Tello and of Inca ethnohistory with Luis E. Valcarcel.

Valcarcel, one of the early *indigenistas*, founded the Department of Anthropology at the Universidad Nacional Mayor de San Marcos in 1946, as well as the Instituto Indigenista Peruano. Peruvian anthropology in the following years developed around these two institutions.

But under Mexican, North American, and French influences, the interest of these institutions shifted from the ethnohistory of Valcarcel to the concept of native communities. The Instituto Indigenista rapidly became bureaucratized and transformed into a government agency within the *Plan Nacional de Incorporación de la Población Aborigen.*

This program involved close collaboration with North American institutions in promoting community development plans. Basically, it sought to promote the incorporation of the native communities into the national commercial sphere. These plans are rooted in the dualistic concept of the society in which the urban sector is the modern one and the native peasantry is backward in its traditional culture. The Instituto Indigenista has a paternalistic approach toward the peasants in both theory and action. In the theory, the situation of the peasants is not seen as a result of their relationship for centuries with the urban bourgeoisie, but rather as a result of an innate in capacityfor modernity. In the bourgeoisie's imitation of the capitalist metropolis, they strive to appear as messiah and savior among their victims.

The development of anthropology in the university was not altogether different at first. The proponents of community development established themselves there too, and the concept of society was dualistic. But continual field experience, an abundance of students from the provinces in the university, and Marxist anti-imperialism have called this concept into question.

Thus, in the 1960's it became increasingly difficult to defend the concept of a dual society and sustain community development plans in the university. Moreover, under the impact of the Vietnam war, the North American institutions which largely financed these projects began to see the peasants not as a backward population to be educated but rather as a potential political force. The parallel with the Vietnamese peasantry, which threatened North American military power, became more evident as guerrillas appeared in various parts of Peru, led by bourgeois leftists and influenced by the Cuban Revolution. So, the peasantry was seen as

an active social group which threatened not just the national bourgeoisie but imperialism itself.

The same institutions which had earlier promoted community development retreated to positions of observation. It was then that the Peru-Cornell project for 'studies of change among Peruvian peoples'[4] began. The purpose of the project was to observe change in rural society on a national level over a period of several years. In one way or another most Peruvian anthropologists have collaborated in this study. But the consequence was the break between a segment of Peruvian national anthropology and those North American universities which were funding the research, a schism also evident on the level of theory.

In the new perspective such notions as imperialism, domination and independence, and peasant self-determination, have become a central part of Peruvian critical anthropology. Alongside such traditional publications as *Revista del Museo Nacional*, *Revista Universitaria* (Cuzco), and *Folklore Americano*, new ones have appeared which promote this anthropological perspective among the students of the universities in Lima and the provinces. These publications include *Campesino* (Lima), *Rikchay* (Lima), *Ideología* (Ayacucho), *Proceso* (Huancayo), and *Wayka* (Cuzco). Many of the articles appearing in those journals focus on the critique of traditional anthropology, the objectives anthropology ought to have, the class situation of the peasantry, peasant movements, and politicization of the peasantry. Most of the authors have close ties to Marxist political groups of varying perspective.

Another group of anthropologists was absorbed by the government bureaucracy following the nationalist military *coup* of 1968. These scholars have been working to implement the peasant program proposed by the military junta.

The foreign financial support of Peruvian anthropology at large, primarily from North American institutions remained constant until 1968 and began to decline subsequently. The anthropology program at the Catholic University in Lima had just begun at this time and that university has been the only institution with substantial foreign support for anthropology in the years since. In this program a third group of anthropologists has tried to grasp and pass on primarily French structuralist anthropology and Britisch social anthropology, including the latter's symbolic pretensions.

4. A variety of publications have come out of this project. An extensive bibliography may be found in Matos Mar *et al.* (1969).

Let us return to the critical group and its discussion of the objectives of anthropology.[5] Their starting point is the complicity of traditional integrationist Peruvian anthropology with imperialist penetration. They focus on the highland peasantry in terms of its class position within the context of Peru as an internationally dependent society, and the peasantry's consequent cultural situation determined in part by a pre-capitalist past and in part by an inevitable lack of automomy within a society dominated by imperialism. Nevertheless, this group assumes the possibility of formulating a new objective for an anthropology committed to the liberation and self-determination of the Andean peasantry:

> The proletarian alternative to bourgeois neutrality must be militant research. This means the work of organic researchers, who work not individually but contributing concretely to a political organization. Social change depends primarily upon being in and exercising power, and that is achieved only in organized fashion. All else is anarchy-inspiring individualism. Militant research requires, moreover, a different publications policy. It is not enough to publish an empirical study. It is necessary to elaborate theory based on empirical work – redefining, creating, or eliminating concepts. On the other hand, the political conclusions derived from research must be presented and discussed with the people who are fighting constantly to make the revolution. In this discussion the researcher will surely be able to learn whether his work is useful or not. Of course, a researcher cannot be the driving force of a political party. He must only hope to be a supporting force and therefore useful. (Montoya, 1971, p. 33.)

Now we come to the central question which emerges from a brief review of the development of anthropology in Peru and its relationship to the peasantry. The intimate connection between anthropology and the interests of the metropolitan and national bourgeoisie has been consistent. Is it possible to convert a theoretical and methodological body, which developed within a class as a tool of domination and exploitation by that class, into an instrument of liberation and self–determination for human groups that do not share the cultural tradition from which anthropology sprang?

Jose María Arguedas, a Peruvian anthropologist and poet whose personal history included participation in both traditions, learned anthropology in order to put his knowledge at the service of the Quechua peoples but failed to reach his goal. For him anthropology was not suitable for

5. Brief information on some of these positions may be found in the article "Una encuesta en las ciencias sociales" (1971), especially the contributions of Montoya Degregori, and Varese. The clearest positions in this debate are perhaps those of Montoya (1973) and Degregori *et al.* (1971).

expressing and appreciating the Quechua worldview. Poetry and the novel proved more valuable to him, but even with these he lost hope as the Quechua tradition and experience were being rapidly annihilated. He committed suicide in January 1970.

Arguedas' despair in the face of the transformation of the Quechua peasantry into a rural proletariat, consumers of plastics and prefabricated culture, by means of alienating urban education and commercial radio, expresses the possibilities. Nevertheless, anthropologists can set some objectives in spite of being anthropologists. With their possibilities of understanding societies, they can contribute to the critical analysis of their own class. Perhaps in this manner they may contribute more effectively to the liberation and self-determination of members of another class and culture. Perhaps there is also a slight possibility of passing on to an exploited and dominated class and culture some tools of understanding of the social future, as well as of communication, so that that class and culture might achieve self-determination.

But any thinking which might formulate and make possible the self-determination of a class or ethnic group like the Quechua peasantry must transcend not only the bourgeois thinking expressed in anthropology but also the contemporary thinking of the peasantry itself. Both reify the peasantry. Anthropology makes it an object of study and manipulation, as an expression of the class which make it an object of exploitation. And peasant thinking, since it explains its lack of self-determination within an ideological context fundamentally influenced by a dependence upon nature, which expresses domination in its transcendent character, interprets the peasants themselves as objects of abstract forces which do not open up to them the possibility of free will to achieve their own destiny.

It would be superfluous to repeat about Mexican social science what has already been said for Peru. From the early moments of the Conquest, from Fray Bernardino de Sahagun to Gonzalo Aguirre Beltrán, we find in Mexico an anthropology which is derived from the ideological premises of the dominant groups and classes, and which serves as the instrument for the social, economic, and cultural subjugation, first of the conquered societies, later of exploited classes.

Nevertheless, it is fitting to comment on Mexican applied anthropology, *indigenismo*, and its ideological premises, since it has had a profound influence upon Latin American anthropology. We can then evaluate some of the criticisms of it which have emerged since 1968.

Mexico is a complex society of classes. When colonial society entered the mercantile phase and later industrial capitalism, the need arose both to raise the technological and acquisitive level of the dependent population and to partially transform it into a proletariat. The exploitation could no longer be carried out merely through salaried and servile labor, but also through unequal exchange relationships determined by the dominant sector of the society. So, it became inherently necessary for the system to expand both the acquisitive level and the productive capacity of the dependent sectors.

In this situation *indigenismo* emerged and sustained itself. What indigenismo terms 'Intergration' into the Mexican nation is not integration of groups which do not already belong to Mexican society and thus have not defined their situation in terms of such a relationship. Rather it is a constant adaptation of dependent groups within the society to the demands of the ruling class of that society.

This opposition between ruling class and adaptable classes is seen in the ideology of *indigenismo* as an opposition between nation and Indians. Both concepts obscure the true nature of the groups in question. Neither is the dominant class a nation nor do Indians exist. The first is self-explanatory; the second perhaps calls for some explanation. If the concept 'Indian' is intended as a term encompassing a cultural group, it is meaningless. The cultural diversity of the so-called Indian groups is such that there is simply no basis for lumping them into a category. Among the Lacandón of eastern Chiapas, the Mixteca of Oaxaca, and the Tarahumara of Chihuahua, there is no common denominator except that their social and cultural organization do not meet the demands of the ruling class of the society.

When the Sociedad Indianista Mexicana was founded in 1910 under the sponsorship of Porfirio Díaz, the real interest of integration was made clear:

> When the mortar has been replaced by the corn mill, when the rude work of grinding the corn has been replaced by the needle and the hook and become productive, we shall have exalted that race. (Quoted in Comas, 1964, pp. 15, 16, from a letter of José Diego Fernández to the founder of the Sociedad Indianista Mexicana, Francisco Belmar.)

This objective was not expressed as a need arising from the economic interests of the dominant class, but rather as the interest of the peoples affected as an abstract historical necessity. One of the fifteen principles of the Sociedad Indianista Mexicana was 'to stimulate the persons of

indigenous race and their friends to promote whatever they believe suitable for the development of our peoples or to inspire the phenomenon of social evolution needed by the Indian's culture.' (Quoted in Comas, 1964, p. 16.)

This tendency of the anthropologists to speak as if they represented the interests of the exploited classes is expressed in pronouncements which approach a comic level, as when Manuel Gamio, the 'founder of anthropology and scientific *indigenismo*' (Comas, 1964, p. 20) writes:

> Our utilitarian and practical tendency consists of arousing public interest in the existence of a huge majority of Mexicans who are unknown in spite of their right to be studied in order to be understood and consciously pushed forward in their social evolution. Only in this way will their incorporation into national life be attained. (Quoted in Comas, 1964, p. 16.)

There would be no point in elaborating here, step by step, on the sophistication and development of applied anthropology. It is sufficient to quote Gonzalo Aguirre Beltrán of the Instituto Nacional Indigenista in 1970, when he tried to briefly refute the arguments of those who criticize the integrationism of the *indigenistas:*

> The condition of the Indian, encased in a caste structure—or if you prefer, a colonial relationship—is untenable in a liberal capitalist class society which postulates as one of its substantive goals equality of opportunity for all those who live within the national territory.
>
> Those who for one reason or another do not fully participate in the rules of the game imposed by the most economically and culturally advanced segment of the population, must change to adapt themselves to the levels of development attained by the advanced group. Those segments of the population which lag behind—in this case the Indians and non-Indians who live in the regions of refuge—are coercively pushed toward progress by the ruling majority group.
>
> The condition of the Indian, as well as that of the non-Indian, is negated by *indigenismo* and its manifest purpose is that one and the other acquire the condition of Mexicans. (Aguirre Beltran, 1970b, pp. 337–338.)

The purpose has not changed; only the statement is more sophisticated, direct, and brutal. The dominant group, declared a majority, reserves the power to itself, supporting its position with an abstract historical need to transform the rest of the society according to its needs, now obscured by the veil of 'the condition of Mexicans.' The efficient architects of the acts of domination are the anthropologists.

In the 1970's various critiques of the Mexican anthropological industry have emerged, especially from anthropologists who have not directly been a part of the integrationist bureaucracy of the institutions of *indigenismo*. Such critics as Rodolfo Stavenhagen, Angel Palerm, Daniel Cazés, Arturo Warman, Mercedes Olivera, Guillermo Bonfil, Enrique Valencia and Margarita Nolasco, approach Mexican applied anthropology from various points of view. The *indigenistas* dismiss them as romantics who do not wish to recognize historical necessity; thus, the *indigenistas* regard themselves as representatives of a greater force, historical process.

It is precisely this historical process and the presumed necessity for action which arises out of it that are questioned by some of the critics. Palerm (1970, p. 303) opposes what he calls the unilinear concept of history implicit in *indigenista* theory and practice. His model is one of multilinear evolution; he accepts the notion that past history is one of a limited series of possible historical patterns. If this premise is accepted then applied anthropology appears to be an historical mistake. 'It has all been and continues to be not only cruel, pitiless, and atrocious, but also unnecessary.' (Palerm, 1970, p. 306.)

The fallacy of the argument no doubt lies in the circularity of its justification. Of course, for those who have economic and social power in a society such as the Mexican one, the rationale does emerge, not from the ideological system which justifies it, but rather from their concrete economic and social interests. These interests are in fact unilinear; they cannot allow a sector to independently pursue its own historical course or permit a reordering of the society on the basis of premises other than those of their own domination.

Warman (1970a, p. 88) criticizes the other pillar of *indigenista* theory, the assortment of ideas expressed in the concept variously called 'national culture, nationality, fatherland, Mexican society, greater Mexican community, and even *lo mexicano*, as opposed to the other chimera known as "indigenous culture"'. He domonstrates that such terms make no sense in terms of the development of *indigenista* thought, but points them out as euphemisms for the dominant class in the country.

Warman is aware of the relationship between anthropology and Western expansion (1970b, p. 11), but he resists placing this culturist term in its context of concrete class in the Mexican situation. He shares this tendency with the vast majority of critics of applied anthropology in Mexico. Nearly all of them stop short of questioning the possibilities

of anthropology as such. Anthropology remains a science beyond concrete societies and their historical possibilities.[6]

Anthropology in Latin America is the instrument of the dominant classes in their relationships with the exploited classes. It forms part of a cultural context derived from bourgeois European thought which reifies potentially exploitable human groups. There are few indications that permit us to see a potential significance for anthropology in the context of the liberation of those who have been its objects, since Latin American anthropology as a discipline had its origin not in the tradition of the European Enlightenment but in the tradition of European imperialism.

Since Latin American anthropology is intimately tied to imperialism, since it is continually redefined in terms of subjugation and justification of the present colonial continuum, it is very difficult to associate it with the negation of the systems of exploitation imposed and the monoculturism cultivated by the dominant groups. Even its critical potential first appears in the reception of European and North American critical anthropology, and is only applied to domestic affairs in the 1970's.

The intimate relationship of Latin American anthropology with imperial interests and classist domination inhibits our consideration of it as potentially anything more than it is. It is more probable that the pre-capitalist tradition, not necessarily a pre-colonial one, the village tradition of intimate relationships among the exploited, the tradition of mutual aid, the tradition which is systematically attacked by the dominant group—and with them the anthropoliogists—might develop into a potential for liberation than anthropology itself. In the best of situations, the anthropologists might dissociate themselves from their class because of their participation, in one form or another, in the tradition of the oppressed.

6. See Warman *et al.* (1970). Bonfil (1970, p. 65) sees the problem clearly:

> The thousand forms of human alienation are our challenge and define our commitment. Our own alienation from the society to which we belong and the culture in which we participate is the immediate and concrete context in which this commitment must be expressed through action. There is no way to liberate others if we remain slaves, or masters; there is no form of redeeming the Indian without liberating our own society, reversing the process of alienation of our own culture. For that we must rely upon the critical analysis that anthropology can make of sociocultural reality. Such is our commitment.

But even Bonfil fails to elaborate on the possibilities of the new anthropology.

References

Aguirre Beltrán, Gonzalo (1970a), Los simbolos étnicos de la identidad nacional, *Anuario Indigenista,* 30: 101–140. México D. F.

— (1970b), Reseña de 'De eso que llaman Antropologia Mexicana' *Anuario Indigenista,* 30: 322–340. México D. F.

Arguedas, José Maria (1972), *Temblar/Katatay.* Instituto Nacional de Cultura, Lima.

Arriaga, Pablo Joseph de (1920), *La extirpación de la idolatría en el Perú y de los medios para la conversión de ellos,* vol. 1. Lima, Colección de Libros y Documentos referentes a la Historia del Perú, serie II.

Avila, Francisco de (1967), *Tratado y relacion de los errores, falsos Dioses ... Quellenwerke zur Alten Geschichte Amerikas aufgezeichnet in den Sprachen der Eingeborenen,* Bd. VIII. Berlin, Hrsg. v. Ibero-Amerikanischen Institut.

Bonfil, Guillermo (1970), Del indigenismo de la revolución a la antropología critica. In Warman *et al.* (1970). Pp. 39–65.

Comas, Juan (1964), *La Antropología Social Aplicada en México.* Instituto Indigenista Interamericano. México.

Degregory, Carlos Iván, Casanova R., Julio W. and Gálvez R., Modesto (1971), El Proceso de la Antropología: Un Esbozo Inicial, *UNSCH-Ciencias Sociales,* 3: 1–25. Ayacucho.

Diez de San Miguel, Garci (1964), *Visita hecha a la provincia de Chucuito ... en el año 1567.* Limas Documentos Regionales para la Etnología y Ethnohistoria Andinas I. Casa de la Cultura del Perú.

Duviols, Pierre (1971), *La lutte contre les religions autochtones dans le Pérou colonial.* Limas Travaux de l'Institut Français d'Etudes Andines, Tome XIII.

Garcilaso de La Vega (1960), *Obras Completas,* 4 vols. Madrid, Biblioteca de Autores Españoles.

Golte, Jürgen (1973), Bauern in Peru, *Indiana,* supplement I. Berlin, Gebr. Mann Verlag.

Juan, Jorge, and Ulloa, Antonio de (1953), *Noticias Secretas de América.* Buenos Aires, Ed. Mar Océano.

Mariátegui, José Carlos (1964), *Siete Ensayos de Interpretación de la Realidad Peruana.* Lima, Empresa Editora Amauta.

Matos Mar, José, *et al.* (1969), *Dominación y cambios en el Perú rural.* Lima, Instituto de Estudios Peruanos.

Millones Santa Gadea, Luis (1973a), Un movimiento nativista del siglo XVI: el Taki Ongoy. In Juan M. Ossio A. (Ed.), *Ideología Mesiánica del Mundo Andino.* Lima. Pp. 83–94.

— (1937b), Nuevos Aspectos del Taki Ongoy. In Juan M. Ossio A. (Ed.), *Ideologia Mesiánica del Mundo Andino.* Lima. Pp. 95–101.

Montoya, Rodrigo (1971), *see Une encuesta en las Ciencias Sociales.* El Objeto de la Antropología en el Perú, *Proceso-Organo de Extensión Cultural de la Universidad Nacional del Centro del Perú* 2 (set.–oct. 1973): 1–4. Huancayo.

Murra, John V. (1956), The economic organization of the Inca State. Unpubl. Ph. D. thesis. Chicago.

Palerm, Ángel (1970), Respuesta, *Anuario Indigenista,* 30: 295–306. México D. F.

Polo de Ondegardo, Juan (1916a), *Los errores y supersticiones de los Indios,* vol. 1. Lima, Colección de Libros y Documentos referentes a la Historia del Perú, serie 1.

— (1916b), *Instrucción contra las ceremonias y ritos,* vol. 2. Lima, Colección de Libros y Documentos referentes a la Historia del Perú, serie 1.

— (1916c), *Relación de los fundamentos acerca del notable daño que resulta de no guardar a los indios sus fueros,* vol. 3. Lima, Colección de Libros y Documentos referentes a la Historia del Perú, serie 1.

— (1917), *Traslado de un cartapacio a manera de borrador,* vol. 4. Lima, Colección de Libros y Documentos referentes a la Historia del Perú, serie 1.

— (1940), Informe sobre la perpetuidad de las encomiendas del Perú, *Revista Histórica,* 13: 125–196. Lima.

Poma de Ayala, Felipe Guamán (1936), *Nueva Corónica y Buen Gobierno.* Codex péruvien illustré. Université de Paris. Travaux et Mémoires de l'Institute d'Ethnologie. XXIII.

Una encuesta en las Ciencias Sociales (1971). In *Textual* 3. Lima, Rev. de Artes y letras. Instituto Nacional de Cultura. Pp. 17–37.

Wachtel, Nathan (1971), *La vision des vaincus. Les Indiens du Pérou devant la Conquête espagnole.* Paris, Éditions Gallimard.

Warman, Arturo (1970a), Comentarios. *Anuario Indigenista,* 30: 85–93. México D. F.

— (1970b), Todos santos y todos difuntos. In Warman *et al.* Pp. 9–38.

Warman, Arturo, Nolasco, Margarita, Bonfil, Guillermo, Olivera, Mercedes, and Valencia, Enrique (1970), *De eso que llaman Antropología Mexicana.* México D. F., Ed. Nuestro Tiempo.

18 Germany: The search for a new ancestor

WOLF LEPENIES

1. The issue of Georg Forster[1]

The crisis of the humanities – which epistemologically expresses itself primarily in the problem of value judgment and, in practice, in the precarious relation of the particular humanistic discipline to its political premises and consequences – cannot be overcome through a reconstruction of disciplinary history or be better 'understood' through such a reconstruction by an attempt to achieve a cumulative effect. It does, however, appear to be helpful to reexamine a fragment of anthropological tradition in exemplary perspective – for on the basis of the needs of the present we must try to learn from the past. Above all, we should test whether or not we are dragging around useless traditions as ballast, that is, whether we have neglected present-day formulations of problems while overlooking earlier attempts to resolve burning issues.

The translation of the passages from Forster's *A voyage round the world* follows the English original, which appeared in 1777 (London, B. White, J. Robson, P. Elmsley and G. Robinson). I would like to draw the attention of the reader to Horst Fiedler's *Georg Forster Bibliographie. 1767 bis 1970* (Berlin, GDR, Akademie Verlag 1971) which had not yet appeared when I finished this essay.

If I have chosen to recall Georg Forster (1754–1794) in this essay, it has not been only to assure myself about an aspect of the German

1. Unless otherwise noted, I quote from Georg Forster's collected writings, published by his daughter and accompanied by a character sketch of Forster by G. G. Gervinus, Vols. 1–9, Leipzig, Brockhaus, 1843. Roman numerals in parenthesis, without mention of the author's name, designate the particular volumes of this edition.

anthropological tradition. Rather, I have also aimed to show, through Forster, the degree and intensity to which the formulations of a problem in the 'classical epoch' of anthropology are based on questions that are of current interest within the humanities. All too often, the summary judgments with which we evaluate the past of a science and the grandiose perspective from which we observe it are not the consequence of clarified judgment. Rather, they are the result of a hastily formed prejudgment which ideally stylizes the past or criticizes it as irrelevant, in consequence of which one may again focus on a history-less present.

2. An underestimated anthropologist: A forgotten republican

The concern with Georg Forster, thus, is an attempt to make a new reading of a little-known chapter in the history of the humanities and to understand it properly. Forster, whom Jacob Moleschott placed on a par with Lessing and Goethe and whom Friedrich Engels called the German Thomas Paine, has long been forgotten in Germany, not only as a republican but also as a 'national revolutionary without a revolutionary nation' (Träger, 1962, p. 630). Nor has Forster, who designated his original field of interest as 'the natural sciences in the broadest sense and, in particular, anthropology', ever been granted the rank he deserves in the history of the humanities. This can be shown by a cursory look at histories and historians of anthropology.

Slotkin, who in his *Readings in early anthropology* lets Herder and Blumenbach have their say, does not look into Forster (Slotkin, 1965); similarly, Harris, who criticizes Lowie for mentioning Christoph Meiners while overlooking Démeunier in his *History of ethnological theory,* is also unfamiliar with Forster (Harris, 1968). A voluminous German sourcebook merely cites one letter sent to Forster by Wilhelm von Humboldt (Landmann, 1962). Lowie devotes one and a half pages to Christoph Meiners (1747–1810) and, although he criticizes him, ranks him with Klemm and Waitz among the 'pioneers' of ethnology. Lowie seems to be unaware of the attack that Forster published in volumes 4 through 7 of the *Göttingisches Historisches Magazin.* There, Meiners, asserting that 'the most anthropomorphous apes are more similar to ugly Negroes than Negroes are to Europeans', had arrived at the conclusion 'that slavery would redound to the advantage of all mankind, wherefore it is more equitable that a small part of mankind suffer than to cause

damage to the rest of mankind by the sudden abolition of slavery' (Ihle, 1931, pp. 52–53, 55). Forster's rebuff in the *Allgemeine Litteraturzeitung* (V, p. 378ff.) must appear much more sensible to us today than all of Meiners' monumental *œuvre*. It certainly would be more fitting to come upon Meiners through Forster rather than vice versa.[2] It should be noted, however, that Lowie does mention Forster. Yet from the following passage, it is not entirely clear whether the reference is to Georg or to Johann Reinhold Forster, his father. 'Travellers who admirably elucidated externals failed to go deeply into native beliefs and customs. Captain Cook was accompanied by such scientists as Banks and Forster, whose observations remain inestimable. But the time spent on his voyages permitted no thorough study of religion or family life' (Lowie, 1937, p. 6). Whether the reference is to Johann Reinhold or to Georg Forster, who in fact complained that on Cook's voyage the study of nature had always been considered a matter of secondary importance (II, p. 373), this reserved judgment does not do justice to either the father or the son. Jean Poirier, in 'Histoire de la pensée ethnologique', nevertheless mentions Johann Reinhold Forster alongside Cook, Parkinson and Bougainville among the 'grands voyageurs'; naturally, there is no mention of Georg Forster (Poirier, 1968, p. 23).

Incidental mentions of Georg Forster are found in references to the forerunners of polygenetic conceptions, where Meiners and Forster are cited in the same breath (Stocking, 1968, p. 39), as well as in investigations of the conceptual history of anthropology.[3] Works that refer to the important role of ethnology in the history of the University of Göttingen, of course, could hardly omit Forster (Plischke, 1931, 1937, 1960).

Full studies of Forster have been made by Erwin A. Ackerknecht and Wilhelm E. Mühlmann. In his article 'George Forster, Alexander von Humboldt, and ethnology', Ackerknecht limits himself to proving that Forster 'anticipated certain trends in ethnology by more than one hun-

2. I am indebted to Krader, 1972, for the knowledge that Marx knew Meiners.

3. For a long time the *Beiträge zur Völker- und Länderkunde*, published by Johann Reinhold Forster and his son-in-law Matthias Christian Sprengel in 1781 in Leipzig, were considered to be the earliest evidence for the term 'ethnology'. Hans Fischer has established that the concept 'anthropographia and ethnographia' *(Menschen- und Völkerkunde)* is already found in Johann Christoph Gatterers, *Abriss der Geographie*, which was published in Göttingen in 1775. The first volume of the *Neue Beiträge zur Völker- und Länderkunde*, edited by Georg Forster and Sprengel, appeared in Leipzig in 1790 (Fischer, 1970).

dred years' (Ackerknecht, 1955, p. 84) but without presenting specifics of Forsterian anthropology. Wilhelm Emil Mühlmann has similarly viewed Forster as a 'forerunner'. In his study on the Polynesian cult associations, *Arioi und Mamaia,* he writes: 'Besides the outstanding observations and scrupulously exact commentary which can still serve as a model for modern ethnography, in both Forsters, the attempt at a theoretical interpretation also occasionally crops up. The more deeply we delve into their work, the more we must we admire how far they have already come.' And he calls special attention to the fact that the source reports on Tahiti, which stemmed from the first half of the nineteenth century, cannot match the reports of the two Forsters. Further, they avoid premature theoretical generalizations, such as those encountered later in W.H.R. Rivers, for instance (Mühlmann, 1955, pp. 15, 25). Nor can other authors who have dealt with the history of the Pacific discoveries overlook the 'field researches' of the two Forsters (Bodi, 1959). Characterizations of the *Voyage round the world* as 'the most readable of all accounts of Cook's voyages', precisely because of their complimentary tone, make us cognizant of the fact that the work in question has not yet been submitted to a thorough ethnological analysis (Smith, 1960, p. 39).

In *Geschichte der Anthropologie,* Mühlmann deschribes the Forsters as the only travelers of the classical epoch 'who at one and the same time felt the need for a theoretical anthropological elaboration and summary of their experiences' (Mühlmann, 1968, p. 53). Nevertheless, he merely notes this 'feeling' and does not undertake a systematic presentation of Georg Forster's reflections, in particular. In another context, there is mention of Georg Forster as one who 'unfortunately died too early' (Mühlmann, 1966, p. 23), but the reverential homage seems to render superfluous any effort at analysis.

However, such an effort has been made by Ludwig Uhlig in his book *Georg Forster, Einheit und Mannigfaltigkeit in seiner geistigen Welt.* It provides an excellent survey of the reception accorded to Forster's work. In his discussion of 'Forster's utter constitutional aversion to any kind of system' (Uhlig, 1965, p. 8), Uhlig attempts a critical introduction to the latter's anthropological views, although he makes too much of the alternation between natural law and historical argumentations in Forster's work.

Today, research on Forster is being promoted primarily in the German Democratic Republic (GDR), The 'Institute for German Language and

Literature' of the 'German Academy of Sciences' in Berlin is preparing, under the general editorship of Gerhard Steiner, a twenty-volume historical-critical edition of the writings, diaries and letters of Georg Forster. These efforts are in line with Georg Lukács' accepted advice to launch 'a real Marxist investigation' of Georg Forster's life and work (Lukács, 1954, p. 21). The Forster in view, of course, is primarily Forster the republican and revolutionary and only secondarily Forster the anthropologist. Indeed, Lukács ranks him among those *not* completely forgotten Germans 'who directly attached themselves to the French Revolution ... because he was already generally known earlier as a naturalist and a publicist'. The task of ensuring that a critical anthropological tradition, for which the name Georg Forster is, I believe, an emblem, will, however, devolve upon humanistic inquiry, incorporating but not subordinate to his republicanism.

The son of a German pastor with English forbears, Georg Forster was born in 1754 in Nassenhuben near Danzig, on Polish soil. At the age of eleven he accompanied his father Johann Reinhold Forster (1729–1798) on a journey which the latter, on behalf of the Russian government, undertook in order to study conditions in the new colonies that had been established on the Volga. During the trip, which led from St. Petersburg to Saratov by way of Moscow and continued as far as Lake Elton, the young Forster learned Russian and the rudiments of botany and ethnology. There he underwent 'two experiences that were of great significance for his later life. First, there was the awakening of his interest in the investigation of nature in connection with the living conditions of human beings; second, he acquired an insight into the system of feudal oppression and favoritism. For when Reinhold Forster in his report exposed the brutal treatment meted out by the *Voivodes* [governors of the provinces in ancient Russia] of Saratov to the colonists, he was deprived of salary through the influence of a cousin of the *Voivodes*. After sojourning with his son in St. Petersburg for ten months, working and waiting for his money, during which time Georg enjoyed the only formal schooling of his life, he decided to emigrate to England'.[4]

4. For biographical data, which in part are cited verbatim, I have drawn on Gerhard Steiner's (1958) introductions to *Georg Forster, Philosophische Schriften,* Berlin (GDR): Akademie-Verlag; Georg Forster (1968) *Werke in zwei Bänden,* Berlin (GDR) and Weimar, Aufbau Verlag; as well as Georg Forster, *Werke in Vier Bänden,* Frankfurt am Main, Insel Verlag.

In 1772 Captain Cook was preparing his second world voyage. When Sir Joseph Banks (1744–1820) refused to participate in this expedition, the English admiralty invited Johann Reinhold Forster to replace him. Forster accepted, but only after an invitation was also extended to his 17-year-old son. 'This three-year sailing voyage, the longest ever undertaken around the earth up to then, led Georg Forster deep into the South Pole region and to many islands not yet trodden by Europeans, affording him the opportunity to experience the undreamt of beauties and to witness memorable occurrences of an alien and unknown world. But it also convulsed him with the terror of dangers and the bitterest of privations plus the fevers of scurvy-like sickness, the effects of which he was to feel throughout the rest of his life.'

After the expedition returned to London, the English admiralty prevented Johann Reinhold Forster from publishing his description of the voyage, under the pretext of making corrections in the style as well as content, inasmuch as 'on the judgment of many, circumstances and events of the voyage dealt with many extremely delicate political and social-moral questions'. Georg Forster, however, who was not bound by contract to the Admiralty, wrote both volumes of *A voyage round the world*, which was printed in London in 1777. These volumes were published in Germany, between 1778 and 1780, under the title *Johann Reinhold Forsters ... Reise um die Welt während den Jahren 1772 bis 1775*. In 1778 Johann Reinhold Forster published in London his *Observations made during a voyage round the world on physical geography, natural history and ethic philosophy*, which Georg Forster translated into German in 1783.

In 1777 Georg Forster journeyed to France. In Paris he presented Buffon a herbarium with South Sea plants and made the acquaintance of Franklin. Then, in the autumn of 1778, he traveled by way of Holland to Germany, 'which he viewed as his homeland without having ever set foot on it till then'. In the same year, Georg Forster became professor of natural history at the Karolinum in Kassel, and in 1780 his father, Johann Reinhold Forster, obtained a professorship in natural history and mineralogy at the University of Halle, where he taught until 1798. In 1784 Georg Forster accepted a call to the Polish university of Vilna for eight years. The following year he married Therese Heyne, daughter of the Göttingen classical scholar Christian Gottlob Heyne (1729–1812), the first University librarian and secretary of the Academy of Sciences. During his honeymoon, Forster received his doctorate in

medicine with a dissertation 'On the edible plants of the South Sea Islands'. In Weimar he made the acquaintance of Goethe, Herder and Wieland. After returning to Vilna, he had to swallow his disappointment at the fact that the Polish government had reneged on its promise to set up a natural science institute. He and his family were forced to live on the edge of bare subsistence. Hence, in 1787 he promptly accepted an offer of the Russian government to become the scientific director of a world circumnavigating expedition. He gave up his job in Vilna and journeyed to Germany in order to ready the expedition. The expedition never set out, however, because of the outbreak of the Turco-Russian war. Likewise, a voyage to the Philippines, which Forster negotiated with the Spanish government, turned out to be a fiasco. When he was offered a professorship at the Petersburg Cadet Institute, Forster rejected it and instead accepted a position as university librarian in Mainz.[5]

> The University library, stuffed with theological writings, was in a bad state when Forster took it over. Moreover, little was done to promote it and it did not offer its head a satisfactory professional occupation. ... What repeatedly heartened Forster, despite all the harassments to which he was subject and what kept alive his intellectual powers despite conditions in Mainz, were the revolutionary movements in Western Europe, especially the events of the French Revolution. Now he wanted to make up his own mind concerning these events, and to arrive at a clear, partisan position. Accordingly he chose the path leading through the countries of bourgeois freedom, through the regions of upheaval through Brabant, Flanders and France. His fellow-traveller, and constant companion during the spring months of 1790, was the twenty-one year old Alexander von Humboldt whom Forster had known since the late autumn of 1789.

The *Ansichten vom Niederrhein (Views from the Lower Rhine)*, published in their original fragmentary form in two volumes in 1791, were the fruit of this journey. Like all of Forster's works, they are rich in anthropological insights. 'I read them as a book on man', wrote Lichtenberg on April 20, 1791, to Samuel Thomas Sömmering, the anatomist and friend of Forster's.

On April 20, 1792, the Legislative Assembly of the French Republic declared war on Austria, which was allied with Prussia. On October 21

5. When Jacob Moleschott asked the 'Mainzer Kunst- und Literatur-Verein' for assistance in sponsoring a Forster celebration on November 26, 1854, the president of the Verein turned down his request with the remark that 'Georg Forster as the librarian in Mainz, accomplished nothing of a special character' (Moleschott, 1857).

of the same year, the revolutionary French People's Army occupied Mainz. At first, Forster assumed a wait-and-see attitude, but then on November 5, 1792, he joined the Mainz Jacobin Club, the 'Society of the Friends of Freedom and Equality', of which he was elected president at the end of the year. In March of 1793 Forster became a deputy of the Thenanian-German National Convention and finally its vice-president. The Convention elected him member of the three-man delegation to represent Mainz, and to propose its annexation to the French Republic. Forster delivered his speech before the French National Convention on March 30, 1793; on the same day, however, the counterrevolutionary army under the command of the King of Prussia reconquered the Mainz Republic. Only Mainz itself held out for four months under siege before it again came under absolutist rule.

Forster, who remained in Paris as a political refugee, fell ill at the end of 1793 and died on January 10, 1794, 'thirty nine years old, in his hotel room in Paris, in the House of the Dutch Patriots, Rue des Moulins. On his bed blanket lay a map of India, the land of his longing, to which he soon wanted to travel'.

3. Tradition and recapitulation

In his exchange with Sol Tax, Robert Heine-Geldern drew attention to a difference–*the* decisive one, in his opinion–between the humanities and the natural sciences. Whereas chemistry can 'forget' its past as alchemy and astronomy can forget its earlier orientation in astrology, the anthropological disciplines are inseparably intertwined with their own history; 'in speaking of the history of "anthropological tradition" we cannot ignore its origins' (Heine-Geldern, 1964, p. 407). This assertion seems acceptable even though it is an oversimplification. It disregards the notion that the status of the humanities, distinguished by its *historicity,* is itself *historically* determined. I should like to explain the meaning of this objection by a reference to Marx.

The antithesis between the natural sciences and the humanities, based on the opposition between human history and unhistorical nature, leads us to forget that man himself is part of nature. This ambiguity of its object, man, who in his history extends the degree of his dominion over nature while at the same time remaining subject to his own nature, to this day characterizes the anthropological undertaking. It should be

recalled that Marx, in the *Economic philosophical manuscripts,* pointed out that this ambiguity is itself historically determined and is therefore dialectically resolvable. In the structure of life under communism, it seemed to him that with 'the genuine resolution of the conflict between man and nature and between man and man', the difference between the humanities and the social sciences would lose its significance. 'Natural science [and] the science of man will be incorporated into each other, there will be *one* science' (Marx, 1968, p. 544). In a similar vein, Marx and Engels spoke in the 'German ideology' of a single science, the science of history, which 'considered from two aspects can be divided into the history of nature and the history of man'. So long, however, as men exist, 'the history of nature and the history of man reciprocally condition each other' (Marx and Engels, 1962, p. 18). The reference to Marx should make clear that a history of the humanities cannot be written without relating it to the real, i.e., to social and political history as a history of domination and servitude. In my opinion, the work of Georg Forster shows the importance of such a combination of anthropology and politics, although this relationship must also be *interpreted.* The observation that Forster's anthropological conception 'primarily bears a political-scientific character' (Steiner) is surely correct, but it remains of little consequence because the specific character of these relations between science and politics, so determining for anthropology, must be made clear. Here I shall elaborate this specific political-scientific character of Georg Forster's anthropology.

If up to now I have spoken of anthropology and employ this term further–apart from individual passages in which it is necessary to speak of 'ethnology' in the German sense of the word *(Völkerkunde)*–I mean therewith that 'all-embracing anthropology' (Heine-Geldern, 1964, p. 407), which is typical of the early 'undifferentiated anthropologists or ethnologists' (Acherknecht, 1954, p. 120). I also use the concept anthropology as a designation for the totality of the humanities (Lepenies, 1971), which today constitute more than a conglomerate of disciplines resembling one another in a methodological and scientifico-theoretical respect. Rather, they are integrating themselves through an increasingly interdisciplinary orientation (Tax, 1955) and are developing 'towards a unified anthropology' (Thompson, 1957).

At first blush this state of affairs seems to make a backward glance at the history of the discipline especially imperative. If the anthropology of the 'classical' period was, so to speak, the universal science of man

even before any differentiations of the anthropological, subdisciplines on the basis of a division of labor and emphasis, today it defines itself as a synthesis of different individual disciplines. Thus, it may seem to represent the 'reflective' stage of a science over against its 'classical' and naive form. This appraisal, however, is nothing more than the expression of a naive evolutionary conception of the history of science. A construction that conceives the history of anthropology as a continuum of which the present forms an end-point may, at first glance, seem to overcome the traditional antihistoricism, but it does so only at the high price of substituting historical one-sidedness for nonhistoricity.

In respect to the eighteenth century, Michel Foucault has convincingly demonstrated how difficult it is for us today, and necessarily so, 'to understand' a stage of the history of anthropology. For example, the later Buffon – because we presuppose particular conceptions of the evolutionary theory as given – appears to us as the ancestor of Lamarck and the precursor of evolution. This, despite the fact that there can be no talk of 'evolutionism' in a time which does not know the concept of *life (la vie)* and is in a position to talk merely of different *living beings* or organisms *(des êtres vivants)* (Foucault, 1966, pp. 137–176). In order to escape the dilemma of describing the history of the humanities as a history of 'precursors' or 'successors', Foucault logically tried to present this 'history' as the sum of discontinuities and to reconstruct an 'archéologie des sciences humaines'.

Naturally, Foucault's consideration leads to a new dilemma, no less grave. No doubt it proves fruitful to break through the naiveté of all the conceptions of a cumulative development of science. Nevertheless, he was not able to formulate any convincing principles according to which the individual layers, the 'socles epistemologiques', of the humanities would be laid bare. These principles, rather, seem to have sprung exclusively from the arbitrariness of one who directs his gaze upon the past of anthropology; in the final analysis, Foucault's perspective appears just as centered on the present time as is the gaze of those champions of a cumulative conception of history against whom his 'archéologie' seems to be obviously directed.

In my view a dialectical concern with the history of anthropology is characterized by what I would like to call 'recapitulation and ctirique'. For one thing, we can learn from the history of anthropology through recapitulation; indeed, this itself is also a consequence of the rejection of

a naive cumulative conception of the history of science. Taking Forster as an example, I would like above all to show this in connection with the problem of the relations between theory and practice in the humanities as well as in the taxonomy of what I shall call the 'experience of the nonidentical'. In bourgeois society the objects, as well as the victims, of this nonidentity are children as against adults, the crazed as against the normal, the savages as against civilized peoples. An historically oriented theory of socialization, psycho- and sociopathology, as well as ethnology are the anthropological disciplines which concern themselves primarily with the problem of nonidentity or, more exactly, with the organization and the working through of nonidentity. For each one of them, obviously, we can hardly talk of 'progress' in this area. The problem of the experience of the alien and that of understanding the alien seems today to have been no more convincingly mastered than it was in the 'classical' epoch of anthropology. Indeed, we can agree with Eliot D. Chapple when he stressed that 'we (speaking for the discipline) have lost our birthright, the use of eyes with which to see, a capacity many earlier field anthropologists had' (*cf.* Goldschmidt, 1972, p. 73).

The renewal of historical attempts to solve the problems which perenially concern the humanities should not, however, lead to an undiscriminating historicism. The *critique* of an anthropological tradition in this perspective is possible and necessary in a twofold way: It must appraise an anthropological tradition in terms of its own epoch and contemporaneously serve as a critique which keeps an aspect of tradition alive precisely in transposing it to the present. What seems to me as paradigmatic for a criticism which neither proceeds in an appropriate *historical* sense nor takes the trouble to function along the lines of renewal are the attempts to see the historical origin of *anthropology* in either imperialism *or* the Enlightenment. The first conception is guilty of an historical oversimplification and suppresses the objective-progressive consequences of imperial and colonial rule. The 'terribles simplificateurs' who, without hesitation, use the entanglement of anthropology with colonialism for the purpose of denouncing the discipline should be remainded of Marx, who certainly cannot be suspected of a complicity with colonialism and who in the *Communist manifesto* set in bold relief the 'highly revolutionary role' which the bourgeoisie has played in history. Therefore, those conceptions which see in the Enlightenment only the glorious past of anthropology and regard the service to

colonialism and imperialism as a perversion of its original progressive intentions are incomplete. On the other hand, a dialectical concern with the tradition of anthropology, when it considers the real social bases of the humanities, must thematize criticism of the Enlightenment. It must do this in order to preserve the Enlightenment tradition effectively against the counter-Enlightenment. This effort to revive Forsterian anthropology should, then, be understood as a critique of the Enlightenment.

4. On the anthropology of the 'classical epoch'

The anthropology of that eighteenth century, which Herder called 'the great century of doubt and agitation', was still viewed by its representatives as an all-embracing science of man, as a natural science and as a particular branch of human learning. Today we are all too likely to grasp this universal science as a necessary product of the pre-division-of-labor period in science and to forget the synthetic achievement and the interdisciplinary orientation which formed the base of such a science. Before proceeding to a closer characterization of the Forsterian anthropology in several of its aspects, however, I shall briefly sketch the views of other eighteenth-century anthropologists (*cf.* in this connection Marquard, 1965, 1961).

The interdisciplinary orientation of this anthropology appears in the combination of the biologically and ethnologically oriented science of man. This is clearly discernible in the aphoristic observations of Georg Christoph Lichtenberg (1742–1799) in collaboration with whom Georg Forster – not without friction – published the *Göttingische Magazin der Wissenschaften und Litteratur* from 1780 to 1785. Lichtenberg, in keeping with his scientific orientation as a physicist, concentrated on the problems of biological anthropology, i.e., on the relation between the human and the infra-animal species. His ethnological marginal notes were written down with the intent to break through native prejudice structures. With regard to Georg Forster, it may be useful to note that some of these aphorisms were composed after he had read the report of Cook's first voyage, which was published in London in 1773 and was reviewed in the same year in the *Göttingischen gelehrten Anzeigen.* Others were influenced by Bougainville's travel book, which came out in Paris in 1771.

For Lichtenberg, man is above all a borderline being,[6] 'half spirit and half matter' (Lichtenberg, 1968, p. 254), 'half ape and half angel' (Lichtenberg, 1968, p. 361); among all the animals of the world, he is nearest to the ape (Lichtenberg, 1968, p. 75). Therefore, one should not spawn any 'overly artificial ideas' of man but should instead judge him 'naturally', by which Lichtenberg means that man is not to be considered as either too good or too evil (Lichtenberg, 1968, p. 433). His criticism is directed against a 'compartmentalizing philosophy' which 'in anthropology sunders the purely animal from the purely rational man', whereas in reality the two are united (Lichtenberg, 1968, p. 859). God made beasts, but man makes himself; he is the product of nature, of society, of himself (Lichtenberg, 1968, p. 896).

Here, Lichtenberg's conceptions border closely on those of Immanuel Kant (1724–1804) who systematically 'grasped anthropology as a theory of the knowledge of man' and divided it into *physiological* and *pragmatic* knowledge of man; whereas the former bases itself on the investigation 'of what *nature* makes of man', the latter expounds 'what *he*, as a free-acting being makes of himself, or can and should make of himself' (Kant, 1968, p. 399). Wilhelm von Humboldt, in his *Plan of a comparative anthropology* (1794) had similarly distinguished between philosophical and physiological anthropology, but only to point out how each must be related to the other. 'In order at the same time to know with exactness how man is, and to judge with freedom whereto he can develop, the practical observation sense and the philosophizing mind must be jointly active' (Heinemann, 1929, p. 13). Even the proposal 'to combine speculative reflection with practical observation' which Humboldt made in his *Theory of human knowledge* and which stemmed from the writing *The 18th-century*, aimed at a unification of the anthropological perspective with the methods of the humanities. In this he agreed with Herder (1744–1803) who also wanted to combine 'metaphysical speculations' with the 'experiences and analogies of nature' (Herder, 1967, p. 9).

With Herder, we can speak of the combination of a philosophical-biological and of an ethnological-descriptive anthropology because he conceived, as a unity, the 'ideas of [man's] nature' and the 'different phenomena ... through which he reveals himself in this round arena' (Herder, 1965, p. 207). Kant's 'ethnographic' orientation is somewhat

6. In another perspective, Helmuth Plessner has projected an anthropology explicitly in terms of the human phenomenon of 'borderline' (Plessner, 1965).

less directly manifest; nevertheless, he too had always stressed the necessity of the experience of the elien for 'knowledge of man'. 'Travel is one of the means of broadening the range of anthropology, even if it is only reading travel books. One must, however, first have acquired knowledge of man at home, through intercourse with his fellow city dwellers or countrymen, if one wants to know what one should seek abroad in order to put it in wider context. Without such a plan (which already presupposes knowledge of man) the world citizen must remain very limited in his anthropology. Herein *general knowledge* always precedes *local knowledge*; if one wants to be ordered and guided through philosophy. If this is lacking, all acquired knowledge can yield only a fragmentary groping, but no science' (Kant, 1968, p. 400).

At one time Forster had opposed this program namely, in his criticism of travel literature.[7] On another occasion he expressed skepticism about theoretical constructions based on casual observation.[8]

These sketches of the anthropology of the classical epoch should also reveal the extent to which this legacy has become anthropological tradition. This applies to Humboldt, who has been resurrected by Chomsky as a theorist of language, as well as to Herder, whose continuing influence is discernible merely by referring to German 'philosophical anthropology' (e.g., Gehlen, 1966). Finally, it also applies to Kant, whose anthropology stands at the center of an anthropological-historical as well as a systematically significant debate (Marquard, 1965, 1971; Hinske, 1966). Only Lichtenberg and above all Forster are excluded from this posthumous fame. Nevertheless, it can be shown that Forster certainly belongs within the framework of the method and theories sketched here and that his work matched, if it did not surpass, them in methodological exactitude and theoretical fruitfulness.

7. Herein he agrees with Johann Friedrich Blumenbach (1752–1842) who in his foreword to the *Collection of rare and remarkable travel stories,* published in Memmingenin 1789, stressed the 'necessity of the severest criticism in the use of descriptions of journeys' and voiced an emphatic 'warning in the use of travel stories' (Plischke, 1937, p. 75).

8. Georg Forster dealt with the problem of 'general knowledge' and 'local knowledge' in his article, published in 1791 in the *Neues Deutsches Museum,* the theme, of course, pervades his whole work (Forster, 1958, pp. 157–172).

5. The limits of prejudice

Forster's anthropological works, especially his ethnographic sketches and travel books, impress us even today because of their lack of prejudice. The extent and the systematic importance of this open-mindedness are strikingly apparent when we compare Forster's views with those of Meiners, 'whose ethnological ideas and suggestions' have justifiably been forgotten. Meiners' views are derived from arguments based on 'the natural inequality of man ... and ideas on the rights of the nobility and of the primacy of white over colored peoples' (Ihle, 1931, p. 12). Meiners tried to set forth the principle of an 'inequality of rights', which stems from the 'inequality of natures', above all in the difference between 'Mongolian' and 'Caucasian' stocks, further separating the latter into a superior Celtic and an inferior Slavic race. This separation won him the ironic sobriquet 'Mongol' from Lichtenberg, who made the point in a letter to Forster. Forster critically reviewed Meiners' anthropological outline with great detail in the *Allgemeine Litteraturzeitung*. Its importance transcends the limits of a review because here Forster makes a fundamental principle of a *materialistic* anthropological approach.[9] Whether we owe the advantages of European morality 'to an innate excellence of our inner and outer organization, or rather to the climate, the position of our countries, the chain of previous events, especially to certain, particular impulses of the passions and mental powers of individual men and some fortunate throws of fate, as, for example, the art of printing, on this matter the argument is not likely to be settled easily. Who is so new in the study of mankind that he does not perceive that everything has depended on circumstances and a necessity begotten by them, and not alone on human beings' (V, pp. 380–381). Forster maintains that had Meiners himself visited the peoples he describes, the judgment he delivered would have been more philanthropic 'than all that which he had gleaned together from countless authors, so different in their interests'. Moreover, one would be able 'to draw a frightening picture of superstitions and stupidities, of avarice and selfishness, of coarseness and stubbornness, of debauchery, arrogance, waste, lack

9. In a passage of the *Voyage round the world* Forster had formulated the principle that the national character of a people was to be judged according to the degree of its culture (II, p. 182); on April 24, 1784, he wrote to Sömmering that he 'would like to have several famous philosophers here right now in order to force from them a recognition in honor of our material half' (VII, p. 231).

of feeling and wickedness if one gathered the characteristic features of European peoples from writers of travel books and historians' (V, p. 381). For this reason anthropology is concerned with 'the destruction of vulgar prejudices' (II, p. 485). It must 'consider each people for itself, ... describe it according to all its conditions, and ... investigate exactly just how it fits into the place that it occupies upon the earth' (V, p. 384).

That Forster was strongly influenced by Buffon on this matter is shown in the following passage: 'It is always a doubtful matter to determine between things of different kinds an order of rank with the intention of establishing their absolute worth. On the other hand if we consider each in his species, the elephant, the horse, the dog, etc. as what they are, should be and can be, an excellent *sui generis* comes to light for each one, which we miss nowhere in nature. Should the human species constitute an exception here?' (V, p. 384).

The extent to which Forster's potential for unbiased observation and evaluation of alien cultures had developed is shown, above all, in his position toward the problem of cannibalism.[10] To begin with, Forster disputed the right of 'philosophers, who have only contemplated mankind in their closets' to pass judgment on phenomena of this kind. He also deplored the conduct of Cook's sailors, who had directly witnessed cannibalistic practices and who were so incensed at the sight that they wanted to shoot the man-eaters: 'They were ready to become the most detestable butchers in order to punish the imaginary crime of a people whom they had no right to condemn' (I, p. 513). Forster, in another passage of his travel book, also cites the historic argument for, if not the legitimation at least the understanding of, cannibalism. 'It is a fact no less surprising than certain that the more we examine the history of almost every nation, the more we find this custom prevalent in the first periods of their existence' (II, p. 78). Nevertheless, a look at the abuses of one's own society becomes an intrinsic reason for avoiding a hasty judgment of wholly abominable modes of behavior: 'But though we are too much polished to be canibals, we do not find it unnaturally and savagely cruel to take the field, and to cut one another's throats by thousand, without a single motive, besides the ambition of a prince, or the caprice of his mistress! Is it not from a prejudice that we are disgusted with the idea of

10. Although it was my original intention, I shall not go into Montesquieu's and Lévi-Strauss' observations on cannibalism, as Jean Pouillon has already drawn attention to them in his article in this volume.

eating a dead man, when we feel no remorse in depriving him of life? ... We have instances, that civilized people ... have committed barbarities without example amongst canibals!' (I, p. 517). Like Forster, but with a distinct anticlerical accent, Lichtenberg argued: '*The Priest:* You New Zealanders are cannibals. *New Zealander:* And you priests are God-eaters' (Lichtenberg, 1968, p. 782).

Forster, however, is not entirely free of the characteristic and distorting perspectives of his own cultural view, which *unconsciously* assert themselves. The following episode, for instance, clearly shows his cultural matter-of-fact acceptance of a pronounced difference between male and female roles, i.e., one can be blunt and straightforward with men, but one must be polite with women, which is wholly inappropriate to the situation. Forster reports how a man and (presumably) his daughter came aboard the *Resolution* in New Zealand. 'The man now pulled out a little leather bag, probably of seals skin and having, with a good deal of ceremony, put in his fingers, which he pulled out covered with oil, offered to anoint captain Cook's hair; this honour was however declined, because the unguent, though perhaps held as a delicious perfume, and as the most precious thing the man could bestow, yet seemed to our nostrils not a little offensive; and the very squalid appearances of the bag in which it was contained, contributed to make it still more disgustful. Mt. Hodges did not escape so well; for the girl, having a tuft of feathers, dipt in oil, on a string around her neck, insisted upon dressing him out with it, and he was forced to wear the odoriferous present, *in pure* civility' (I, p. 163; italics mine).

The limits of open-mindedness are most clearly exhibited in the sexual domain. Nevertheless, we can note that here, too, Forster made an effort to work out reflectively a perplexity that on occasion was not to be hidden, as for instance with the dances of the native women of Ulietea, which Cook described as 'indecent'. Forster, too, admitted that they might be 'construed into wantonness; but they were entirely free from that positive degree of gross indecency which the chaste eyes of English ladies of fashion are *forced* to behold at the opera' (I, p. 400).

Nevertheless, the detached portrayal of the alien is convincing throughout Foster's writings. He never separates the reflection from his own culture, as indicated by the observation on the Tahitian, Omai, who was taken along to England. There 'he has been considered either as remarkably stupid, or very intelligent, according to the different allowances which were made by those who judged of his abilities' (I, p. XIV, XV).

Omai, whom Captain Furneaux had brought back to England and who had returned to Huaheine on Captain Cook's third voyage, took along with him for this homecoming, 'a portable organ, an electrical machine, a coat of mail, a suit of mail, a suit of armour and a puppet show' (Plischke, 1960, p. 99). Omai thus behaved like those Europeans who searched among savages for curious objects for their galleries. From an alien looked at with astonishment, he had turned into an astonishing visitor, indeed into an anthropologist, to whom as Forster writes, one had exposed the 'artificial wants' of civilization.

Before I attempt systematically to substantiate the value of such Forsterian observations, I should like to stress that the aforementioned examples go far beyond the limits of the anecdotal. In such anecdotes there are hidden insights which we evade by interpreting them as the reports of an alien who becomes a savage in our eyes but who, in his perspective, distances himself from us while he tries to bring the savages closer to us.[11]

6. On the relation of theory and practice

In the preface to *Voyage round the world* Forster observes that the British legislature had not sent his father, Johann Reinhold, 'as a naturalist, who was merely to bring home a collection of butterflies and dried plants'. Rather, they expected from him 'a philosophical history of the voyage, free from prejudice and vulgar error, where human nature should be represented without any adherence to fallacious systems, and upon the principles of general philanthropy; in short, an account written upon a plan which the learned world had not hitherto seen executed' (I, p. IV). Since the father, as mentioned, was prevented from writing this travel book, the son felt obliged to write 'a philosophical recital of facts' concerning the voyage. At the beginning he offers a critique of system building and empiricism in ethnology. According to Forster, 'the phylosophers of the present age, to obviate the seeming contradictions in the

11. We can establish how the aesthetic difference appears even in scientific and political science programs directed toward the destruction of prejudices in the articles *'espèce humain'* and *'nègres'* in the French Encyclopedia. Even Herder is not free of it when he asserts that everyone is struck by the fact 'that the region of the well-formed peoples is the middle region of the earth, which like beauty herself, lies betwixt two extremes' (Herder, 1967, p. 226). Aesthetic categories seem to be most difficult to apply to the relativism problem complex.

accounts of different travellers, have been at the trouble to select certain authors in whom they have placed confidence, and rejected as fabulous the assertions of all the rest. Without being competent judges of the subject, they have assumed a few circumstances as facts; and wresting even those to suit their own *systems,* have built a superstructure which pleases at a distance, but upon nearer examination partakes of the illusive nature of a dream' (italics mine) (I, p. XI). Then, a sudden change occurred in this system-building which had no experimental base whatsoever: '... they raised a general cry after a simple collection of facts. They had their wish; facts were collected in all parts of the world, and yet knowledge was not increased. They received a confused heap of disjointed limbs, which no art could reunite into a whole; and the rage of hunting after *facts* soon rendered them incapable of forming and resolving a single proposition; like those minute enquirers, whose life is wholly spent in the anatomical dissection of flies, from whence they never draw a single conclusion for the use of mankind, or even of brutes. Besides this, two travellers seldom saw the same object in the same manner, and each reported the fact differently, according to his sensations, and his peculiar mode of thinking. It was therefore necessary to be acquainted with the observer, before any use could be made of his observations' (I, p. XII; italics mine).

It is important to point to the fact that Forster, on the one hand, was able to perceive and describe the new, alien and nonidentical, as such, and, on the other, nevertheless to integrate it into his own experience. I should like to designate the first capacity as 'serendipity', in the sense of Merton, who by the 'serendipity of research' understood 'the discovery, by chance or sagacity, of valid results which were not sought for' (Merton, 1966, p. 103). But Forster resisted the temptation to exoticise the discoveries made 'accidentally' and by so doing remove them from experience. Forster tells of a midshipman who was present on the occasion of a marriage on Tahiti and witnessed a number of ceremonies performed. Upon being entreated to give the particulars, he confessed 'that though they were extremely curious, he could not remember one of them, and did not know how to talk about them'. Here, the vocabulary of description shrinks to the utterances of the 'curious'. In this connection Forster ruefully observes, 'It is a pity that no intelligent observer was present, who might have at least related what he had seen' (II, p. 89). For Forster, the category of 'relating' is a highly reflective one; in the act of *relating*, the purely picturesque is worked through. It remains something alien which is, at the same time, accessible to

experience. It does not become something incomprehensible–to be surpressed as quietly as possible in order to exonerate ethnocentrism.

An achievement, peculiar to Forsterian anthropology and worthy of emulation, is the avoidance of what Lévi-Strauss has called the ethnologist's dilemma, '... critical at home, but conformist abroad' (Lévi-Strauss, 1955, p. 354). This particular achievement consists in the way Forster's anthropological works and reflections complement each other.

Forster clearly perceived that the problem of 'distance' between civilized observers and the uncivilized-appearing objects of their observations could by no means be removed by empathy and certainly never without distortion. In fact Forster, on April 13, 1793, wrote his wife that it requires a 'cold philosophy', but what is expressed in this program is already the resignation of the republican run aground and is by no means a reflection of Forster the anthropologist. Rather, in his preface to *Voyage round the world* he stressed how in his ethnological description, he had yielded to his feelings inasmuch as he was not free of human weaknesses and wanted every reader to know as exactly as possible 'the colour of the glass through which I looked' (I, p. XIII). Each observer has his own way of seeing and his specific 'film over the eye' and as in Thiery de Mononville's *Traité de la culture du Nopal et de l'éducation de la Cochenille* (1787), all that is required in reading travel books 'is to be able to develop the character of the observer immediately from his observations and adventures; one sees all the more truly and more clearly, the more exactly one can calculate the refraction which is peculiar to the medium through which one perforce must see' (V, p. 328). Moreover, 'inquisitive and truth-loving readers' will feel the desire 'for the want of one's own observation, to be guided in as many viewpoints as possible by what others have seen, and to compare their reports with each other'. This is why Forster, in his article on Tahiti which appeared in 1780 in the *Göttingisches Magazin*, made it his scientific premise to pass on many observations 'which either are contrary to ours, or are wholly lacking in our works' (IV, p. 205).

One can measure the accomplishment represented by Forster's dual attempt to avoid making prejudgments while at the same time refusing to succumb naively to the possibility of an unbiased, but also non-evaluative, reflection if one recalls Karl Mannheim's radical, but abortive, attempt to resolve the problem of relativism through so-called 'relationism'. Mannheim believed in 'the assimilation and transcendence of the limitations of particular points of view. [Such "totality"] represents

the continuous expansion of knowledge and has as its goal not achievement of a super-temporally valid conclusion but the broadest possible extension of our horizon of vision' (Mannheim, 1965, p. 106).

Forster's attempt to develop a *dialectics* of the *particular and the general*, as indicated in his article 'On local and general civilization', anticipates Mannheim, but confronts the normative problem more directly. Forster not only formulates the question of perception-distortion programmatically, but also holds fast to it in actual usage. Value judgments which he maintains or negative evaluations of alien customs which he cannot suppress are relativized by him through qualifications such as 'according to our understanding', 'in our eyes', 'our native notions', but he does not surrender the normative conceptions of the civilizational process. Forster begins with the experience of diversity for 'what man could become is what he has become everywhere according to the measure of local conditions. ... Thus nowhere has he become all, but everywhere something different, which promises information to the mind of the investigator, if he reflects on the fates and destinations of his species. ...' (Forster, 1958, p. 157). In another passage, he merely describes 'the beautiful spectacle of the diversity of our species' (IV, p. 275), the 'partial disharmonies and eccentricities' (V, p. 231), not only conceded but needed. He shows that the empirical sciences not only provide the impression of the diverse, but they also reveal 'general nature and that of man'. On the one hand, then, Forster pursues the political implications of this perspective in which the national states are no longer assigned a role worthy of mention within world civilization; indeed, 'the terrible revenge of fate will pursue a nation which does not seize the offered opportunity of casting aside the yoke of a mere local mechanism and to stride forward to that higher freedom of the spirit, which knows neither father nor mother' (Forster, 1958, p. 164). On the other hand, however, he clings to the difference of Europeans 'from the other races of man', for the latter will 'one day receive back their own ideas newly minted with the stamp of generality and the numerous areas of European development, trading posts and conquered provinces of both continents combined will spread the light of reason where before it was received only in subdued, colored rays' (Forster, 1958, p. 162).

But this is not the Europe-centered residue of an otherwise cosmopolitan anthropology. This is not the same as Hegel speaking of Negroes as 'a nation of children' not emergent from their disinterested ingenuousness, permitting themselves to be sold 'without any reflection whatso-

ever as to whether this is right or not', while in contrast the world 'interests' the European, and accordingly he pushes forward 'into the particularities of the world ... in order to demonstrate its inner rationality' (Hegel, 1970, pp. 59, 63).

One, then, cannot consider Forster a Europe-centered anthropologist of the 'classical epoch', i.e., of the Enlightenment; he made the critique of reason his program and did not play off the rationality of Europe against the nonrationality of non-European peoples. Accordingly, for him the 'not-yet resolved problem of humanity' indeed consists in the rule of reason, yet it is necessary at the same time to maintain the 'unimpaired sensitiveness of feeling'. We are able to withstand the 'adventure of enlightenment only when we combine reason and feeling', for 'before reason ripened in us, we followed the tug of feeling, and woe to us if with its denial we relinquish the error that hallowed our childhood' (Forster, 1958, p. 164). Up to the end of his life Forster never relinquished his efforts to demonstrate this 'dialectic of enlightenment'; as late as April 16, 1793, he wrote to his wife from Paris that conflagrations and floods, the damaging effects of fire and winter were as nothing compared to the 'havoc that reason will beget. Mark well, however, reason without feeling. ...'

When we speak of the 'all-embracing' anthropology of the 'classical epoch' and of the 'undifferentiated' anthropologists of the most important of the humanities, we are thinking of an anthropologically unified science to which this unity, as it were, was allotted because the trend to the division of labor had not yet comprehensively asserted itself. Thus, we forget that already in the eighteenth century, the tendency to specialization, to which Forster himself alludes, was further developed than we generally assume. We forget, however, that it by no means involved a 'unified science' but rather the holding of different anthropological systems in equilibrium. This 'balance' and the specific role it played can be demonstrated in the work of Forster, when we first differentiate anthropological and ethnological statements and then relate them to each other.[12]

12. A further reference to the 'topicality' of the Forsterian partisan discernments. The anthropologist Rudolf Bilz, in his study on 'Subject-centrism in the experience of fear', has subtly interpreted the fear-creating effect of cannon shot (Bilz, 1971, p. 129ff.). Forster's travel book is replete with scenes in which fear is instilled in the natives, come upon for the first time, by a discharge of cannon over their heads–material enough for a more exact investigation of the technique of creating fear in the precolonial and colonial epochs.

Traditionally, anthropological arguments serve rule-legitimating functions; this explains the aversion felt, for example, by Marxist-influenced scientists toward anthropological theories. Marx has convincingly proved this in theorems at whose center stands the category of man. Attempts to prove that aggression, envy, war, jealousy and other evils are characteristics peculiar to man cannot be designated as false from the outset; as a rule, however, the upshot of all this is an undercutting of specific socioeconomic attempts at explanation and thereby a considerable lessening of the chances of their successful praxis. In Forster, however, the introduction of anthropological theorems is progressive because it depends less on comparison and, consequently, less on a tendentious, hierarchized differentiation. Instead, it aims at a coalescence of the manifold of human attributes. This applies to man's general, basic drives, e.g., the love of peace, which warrant an altogether positive evaluation, as well as to human vices and follies which, according to Forster, look alike everywhere. The combination of anthropological theorems with elements of ethnographic description enables Forster to see preserved in the 'variations of human nature' the complete identity, for instance, of the original forms of thought and, at the same time, to demonstrate with a most felicitous formulation from the Cook article 'that the nature of man is no doubt climatically different everywhere, but *specifically the same* on the whole, according to the organization as well as in relation to the drives and the course of its development' (V, p. 150; italics mine).

7. Experience of the alien and identity

To discover 'reason also in the figure of an American savage' (IV, p. 203) was by no means something taken for granted in the eighteenth century. Such a view aroused violent criticism and suspicion. In one of the reviews of Georg Forster's *A voyage round the world* which appeared in the supplement to the *Göttingische Anzeigen von gelehrten Sachen* of March 7, 1778, we read: 'Herr Forster makes observations, indeed, which one vainly looks for in Cook's account of the voyage; but these consist for the most part in effusions of sentimentality and in panegyrics to alien virtues, not always clear of suspicion which would make a better impression on many readers if they did not so often recur, and almost each time ... combined with allusions to the cruelty, hardness and

incivilities of Europeans.' Thereupon, Forster, in his 'Answer to the reviewers', which appeared on March 30, 1778, asked why panegyrics to virtues should not be sung 'wherever they may appear, in the beggar's hut or in the palace in Europe or in Tahiti ...'. Forster stressed that for him it was a matter of countering the prejudice that 'we in Europe alone possess all virtue, and attribute to savages nothing but misdeeds and an evil nature. Therefore I show real virtues among savages, where in similar cases depravities reign among polished peoples'.

The reference to the principle of observation whose point of departure is that 'all the various tribes of men [are] entitled to an equal share of my good will', as Forster formulated it in his introduction to the *Voyage*, was necessary in order to recall the historically conditioned resistances against such an enlightenment-minded program. To Forster, a mode of experiencing the alien, determined by premises of this kind, is closely bound up with the rejection of the legend of the 'good savage'. I make this reference, which concerns not only Forster but also others of his generation–in Germany, Kant, in France, de Jaucourt and Diderot (Hubert, 1923, p. 173)–in order to call in question specific presuppositions of the counter-Enlightenment which tries to stamp a particular image on the Enlightenment. The counter-Enlightenment image ascribes to the Enlightenment the aspiration 'to construct human relationships anew, on the basis of reason free from all bonds of tradition and prejudice' (Luhmann, 1970, p. 66) and emphatically projects against the eighteenth century which is identified with Rousseau–regardless of how he may be understood–a 'philosophy of pessimism and of the seriousness of life' (Gehlen, 1961, p. 59). Hence it is necessary to counter the legend that the eighteenth century can be characterized outrightly by 'the old optimistic view of primitive peoples' (Jettmar, 1964, p. 272). We must forge a correct link to the tradition of the Enlightenment by recalling its faculty for thinking skeptically *vis-à-vis* its own premises. Only in this way can we counter the attempt to fabricate a tradition of Enlightenment anthropology with the intention of promoting the tendencies of the counter-Enlightenment.

Alexander von Humboldt has described how 'through Forster's charming portrayals of Otaheiti ... a general interest, which I could describe as suffused with longing was aroused especially in northern Europe for the islands in the Pacific Ocean' (Plischke, 1937, p. 54). This 'interest suffused with longing' can be traced, if at all, only to a highly selective reading of Forster's travel book: 'That this ideal image of a "blessed

isle" spread in Europe, *notwithstanding* the fact that the original reports of both Forsters, portray a positively "earthy" society and culture, is one of the remarkable phenomena of European intellectual history. It can only be explained in my opinion, by the fact that the romantic movement was incapable of an adequate and realistic grasp of the exotic world of peoples: *one wanted* to see an ideal Utopia, and so one saw it' (Mühlmann, 1955, p. 15). The onslaught of the counter-Enlightenment is today, as before, on a naive and inaccurately labeled 'Rousseauism' of that kind. But it should be noted that in the *Conjectural beginning of human history* (1786) Kant, who dedicated an important work to Rousseau, treated with irony the 'yearning, stimulated by the Robinson Crusoes and reports of voyages to the South Sea Islands' and demonstrated 'the futility of this desire to return to that age of simplicity and innocence' (Kant, 1968, vol. 9, p. 101; *cf.* Horkheimer Adorno, 1944). And, as is well known, Rousseau himself had already made the point in his second Discours, in response to such critics as Voltaire.

Forster's rejection of the legend of the good savage is systematically connected with a normative theory of civilizational development which is discernible in him although it is hardly elaborated. The emphatic ejaculation in the writing 'A look into the whole of nature'–'How beautiful is this constructed nature!' (IV, p. 326)–attests to a 'back to culture!' program as Arnold Gehlen has tendentiously formulated it *against* the eighteenth century. But this is a possibility precisely of this epoch itself inasmuch as for Forster the constructed is second nature, culture, and it is his conviction that 'there is no purely animal state of nature' (V, p. 150). 'Not only a lack of knowledge but also a political interest can be imputed to those who cling to such a notion in the face of all experience to the contrary', since after all 'the happiness of the tame slave is as much an invention as is the happiness of the free savage. Both, instead, should be measured in terms of the state of the generally achieved morality and therefore perforce be deemed unhappy' (VI, p. 286). European culture, particularly the scientific, surpasses any other (V, p. 383), and the human being becomes authentically human only in civilized life (IV, p. 184). Therefore, the desire to play off savage life 'destitute of that superior knowledge of which to our shame be it spoken we do not always make the best uses' against the bourgeois condition is viewed as arrant sophistry, for 'if ever the preeminence of a civilized life over that of a savage could have been reasonably disputed, we might from the bare contemplation of these miserable people, draw

the most striking conclusions in favour of our superior happiness'. This, in turn, imposes upon the civilized peoples of Europe the paramount commitment of perceiving this obligation toward the 'primitives' (II, p. 386). Indeed, 'it is much to be lamented that the voyages of Europeans cannot be performed without being fatal to the nations whom they visit' (II, p. 258).

Forster espouses a normative theory of civilization oriented to the state of development of the most advanced European nations; nevertheless, he does not lapse into any kind of ethnocentrism. He criticizes European conditions in terms of his observation of 'primitives', but he does not succumb to any myth of 'savage nobility'. The prerequisite for this is Forster's conception of individuality as an anthropological category, as a feature that is ascribed to each individual human being; individual deviations from generalizable features of conduct are ascertainable among all peoples. Accordingly individuality cannot be taken as the measure of a state of development. In contrast, the colonial ideology was bound up with the notion that 'primitiveness' and 'individuality' are mutually exclusive. Scruples about slavery could hardly become clear so long as the conviction prevailed that one Negro was like another (Bolt, 1971, p. 142). Conversely, then, the emergence of a category such as 'individuality' *vis-à-vis* phenomena of primitive cultures marked the first step toward the demolition of ethnocentric prejudices (Lowie, 1928).

Forster's achievement, above all in the *Voyage*, supersedes all other travel accounts of the time. In New Zealand, for example, some sailors from Cook's crew–after the natives had been correspondingly remunerated–had received Cook's permission to have sexual relations with some unmarried women. Forster utilizes this fact not only for an alternating criticism of the 'barbarians' and 'our people', but he also hastens to confess 'that some (among the women), however, submitted with reluctance to this vile prostitution' (I, p. 211). When, later, on Ulietea, a similar 'lubricity' is confronted, he presumes that 'as at Taheitee, it is highly probable, that we should have observed no other prostitutes than such as belonged to the lowest class of people' (I, p. 457).[13] Fortser

13. In this and the two following paragraphs I employ the concept 'anthropology' exclusively in the sense of biological anthropology as a discipline which above all deals with the drive-structure of man and which is closely related to human ethnology.

In a passage of Johann Reinhold Forster's 'observations', the following remark is found on the relation between social stratification and morality: 'Among those

believes that in every people one can find 'individual physiognomies' which do not allow us to assume much goodness and conversely 'that it is possible even in a slothful people to discover individual industrious, diligent people' (II, p. 415). Forster's freedom from prejudice is most closely connected with his abstention from generalizations. He is decisively opposed 'to deducing a general character of the natives' (IV, p. 172) and to drawing sweeping conclusions from what to him was the shameless conduct of the native women who without adieu, trafficked carnally with Cook's sailors: 'It would be singularly absurd, if O-Maï were to report to his countrymen, that chastity is not known in England, because he did not find the ladies cruel in the Strand' (II, p. 54).

Alexander von Humboldt therefore designated an important aspect of Forsterian anthropology when in the 'Kosmos' he stressed: 'Everything which in the prospect of an exotic nature can vouchsafe truth, individuality and concreteness is found combined in his works' (Plischke, 1937, p. 55). How highly one should appraise the individualizing mode of consideration at this time becomes clear when one recalls the hope expressed as late as 1906 'that the research of the individual among native peoples, up to now almost completely overlooked, would begin'.[14]

Forster, through experiences of the alien, *relativized* conceptions of his own culture and *criticized* the civilizational development of alien cultures on the basis of normative principles. But above all, he submitted them to a radically individualizing valuation. For that reason he was also concerned to see in particular nations and races the individual personality come into its own *vis-à-vis* the national character and to grasp particular countries themselves as individualities. 'On the basis of these very different individualities, if we compare them and separate the general from the local, we develop a more correct conception of mankind', we read in the preface to 'Sakontala' (IX, p. 167). To be

---

of high rank, and among the lowest class of inhabitants examples of selfishness and immorality of all kinds were by far more frequent than among the middle estates' (Forster, J. R., 1787, p. 319). The beginnings of such a 'bourgeois' perspective are also discoverable in Georg Forster, but, according to my survey, no generalizable conclusions can be drawn therefrom.

14. P. W. Schmidt, 1906, p. 640. Schmidt had cited Stephan's *Beiträge zur Psychologie der Bewohner von Neupommern* (1905): 'I was altogether very astonished to find *in each* of these "savages" a sharply delineated individual according to character and ability, in exact contrast to that view which conceives a primitive people as an undifferentiated herd and differentiation only as an occurrence arising from education and culture.'

sure, man must first become rational 'in order to perceive his true advantage in the welfare of the other' if he is to arrive at the 'lofty recognition of human rights in others' (IV, p. 63) 'to discern in the receptiveness to others a source of knowledge and in addition to his individual sensibility to remain open to the mode of conception of others' (IV, p. 275).

Forster grasped the experience of the alien as the model of the process of identity formation. We do not become ourselves when we choose to be other than what we are. Rather, for the formation of our Self we require the experience of Another, in which we grasp this Other as Self and, at the same time, the different from our Self. Forster, exactly like Rousseau, grasped the occurrence of culture contact as that quotidian experience of the alien which permits us to assert ourselves *vis-à-vis* it and thus helps us to arrive at the formation of our own identity. Accordingly, we can interpret ethnocentrism as an aborted process of identity formation along with that species of relativism which believes that it must altogether renounce valuations: 'Toute culture, tout homme ait besoin d'une 'dose de barbarie', soit contraint de nier plus ou moins l'autre pour s'affirmer soi-meme et renforcer sa foi en ses propres valeurs' (Aron, 1970, vol. 2, p. 946).

Forster, then, attempted to avoid the ethnologist's dilemma, which Lévi-Strauss has made his theme, precisely by *not* resolving the ambiguity of anthropological work but rather by accepting it as the prerequisite for every science of man. Even Lévi-Strauss' 'rebuke' of Sartre – according to which the latter does not understand savages precisely at the point where he thinks like them, whereas he, Lévi-Strauss, comprehends them to the degree that he does not think like them (Aron, 1970) – is initimated in Forster, when, in a criticism of the Abbé Graudin's 'Voyage en Corse' (1787) he states: 'We could almost like to believe that the highest culture as regards the spirit of observation is again at one with the raw state of nature because the gaze of man who is used to living in the great world is as quickly distracted as the gaze of the savage and never lingers on the most important objects longer and often not so long, as one the most trifling' (V, p. 326).

8. Science of man and politics

Forster's importance, of course, goes beyond the fact that he grasped how a rational *science* of man presupposes a rational, human *society* and that anthropology and politics are not to be separated from each other. Forster himself did not hold to the view, current in his time, that a science of man worthy of man would be imagined only as a *utopia*. Rather, he tried to advance this insight in practice. Thus, as a republican and adherent of the Revolution, he ran aground in Germany, as did many others, who wanted to make theory operative in practice. They 'find me detestable now that I really set to work according to the principles which when on paper were deemed worthy of applause', he wrote on January 1, 1793, to his wife.

It would be instructive to study how Forster's anthropological views changed as political maxims became a guide to practical action. It is, however, quite clear that the limits of the Forsterian understanding of politics also mark the limits of his anthropology: The history of discoveries was promoted by the mercantile spirit, and boundless praise was heaped on Cook because he never suffered 'that one ... should go unpunished for encroaching upon the generally recognized rights of property, sacred even to savages' (IV, p. 131). On July 19, 1793, Forster reported to his wife on a 'good German book' that had been a source of great joy to him, namely, *On man and his conditions*. Forster did not know the author of the anonymously published work, Carl Wilhelm Frölich, 'a German socialist utopist of the eighteent century' whose book 'on bourgeois philosophical historiography was fully passed over' (Steiner). Forster agreed on many points with the author whose 'political ideas of the community of property', of course, he could not share and considered 'alien to my view'. This also marks the limits of Forsterian republicanism.

Similarly, there is the stunting influence of the commercial orientation in the writing of Abbé Raynal on both Indies, in which commerce connotes the 'communication des peuples'. Commerce is the source of all progress and the motor of development. Whereas war destroys, commerce works creatively, since it is a science, and the expression 'trade war' *(guerres de commerce)* is inadmissible: 'Quel mot contre nature!' (Raynal, 1781, p. 163). The extension of commerce which revolutionizes customs as much as the sciences promoted by men, could effect 'a reciprocal influence between the peoples of the earth and of

the sea, an equilibrium of industry and power, which would make them all communicate together for the general utility. ... The different states would have this freedom of exportation and importation which must reign among the provinces of the same empire' (Raynal, 1781, p. 104).

The 'general welfare', of course, to which the colonies, no longer established in the 'spirit of conquest' but in the 'spirit of commerce'–as the French Encyclopedia has it–were to contribute, was, in truth, the welfare of the metropolis, although this was neither the exclusive nor the deepest intention of the philosophes. The general interest that commerce alleged to promote turned out to be the interest of the bourgeoisie.

This exercise in the tradition of eighteenth-century anthropology makes visible the limits of a science of man, as Georg Forster developed it. We can all reflect on its subtleties, and we can still learn from them. But we must point out its limits in order to avoid the illusion that we have already stepped beyond them. The dilemma of this backward glance on a forgotten fragment of German anthropological tradition lies in this fact: To have recognized the deficiencies of a tradition without having overcome them and to be loyal to the same tradition whose principles themselves demand that they be overcome.

Appendix: Kant vs. Forster

Here i should like to show that Forster's anthropological conceptions are in no way in accord with Kant's and also to point up in their notable debate Forster's characteristic understanding of the relation between science (of man) and politics.

In 1775 Kant announced his lectures on physical geography under the title 'Concerning the different races of man' (Kant, 1968, vol. 9, pp. 11–30). Herein he tried, in the manner of Buffon, to show that all human beings on the earth belong to one and the same natural species because–despite all differences–they can fruitfully beget children with each other. This unity of the natural species, according to Kant, can be derived only from the *origin from one stock*. He did not term Negroes and whites different species, but different races of men, which obviously should not be traced back to distinct 'local creations'.

Ten years later, in the *Teutsche Merkur*, Kant published his 'Definition of the concept of a human race': 'The knowledge of the new travels

spread concerning the varieties in the human species, have up to now contributed more to stimulate the mind to conduct investigations on this than to satisfy it. It is very important first of all to have well defined the concept which it is desired to clarify through observation, before one interrogates experience concerning it. For one finds in it what one needs, only when one knows beforehand what one should be looking for' (Kant, 1968, vol. 9, p. 65). Kant had also expressly defended the 'oneness of the stock' in this article and defined race as the *'class difference of animals of one and the same stock, to the extent that it is inexorably inheritable'*.

In the *Teutsche Merkur* of 1786 Forster submitted Kant's views to a sharp critique with his contribution entitled 'Something concerning the human races' (IV, pp. 280–306). Forster counterposes the observations 'of a mere, yet perceptive and reliable empiricist, to the overly-made up ones of a partisan dogmatist....' Directed above all toward Sömmering's 'Concerning the physical differences between the Negro and the European', which had appeared in 1785, Forster clings to the polygenetic conception. 'If ... each region produced the creatures that were suitable to it, and indeed in their reciprocal relationships which were indispensable to their security and preservation, how come that defenseless man should constitute an exception here?' (VI, p. 301). Forster can consider it neither improbable nor incomprehensible 'that two different stocks and perhaps a sufficient number of individuals from each of them, have arisen as autochthons in different regions of the world'.

Kant replied to Forster's criticism in 1788 in 'On the use of teleological principles in philosophy', and from the viewpoint of methodology, he repeats his critique of the purely empirical traveler and his account, 'particularly when it concerns a coherent cognition from which reason is to make something for the purpose of a theory' (Kant, 1968, vol. 8, p. 141).

Although the dispute over the question of the definition of the concept of race–which, of course, goes far beyond this theme–was discussed in a characteristic way in the Nazi period and even gave rise to a controversy among anthropologists of that epoch (Blome, 1939, 1943; Mühlmann, 1968), I shall not touch upon it here. I am more interested in showing that, in this debate, Forster contributes a noteworthy viewpoint to the relationship between anthropology and politics.

At the end of his article 'Something concerning the human races',

directed against Kant, Forster claims that scientific knowledge should not be molded according to principles of political expediency, even in the pursuit of humanitarian aims. Hence, the polygenetic conception, which at first glance appears as a possible foundation for the legitimation of slavery and colonialism, need not fear the following reproach from monogenists: 'But by separating the Negro as an originally different stock from the white man; are we not cutting the last threads through which this mistreated people is connected with us, and still found some protection and mercy before European cruelty? Let me ask, rather, whether the idea that Blacks are our brothers has anywhere, even but a single time, caused the slave driver to drop his whip? Where is the bond, no matter how strong, that can prevent degenerate Europeans from ruling over their white fellow men as despotically indeed as over Negroes?' (IV, pp. 304–305).

In his article Forster anticipates recent realizations that colonialism and anticolonialism are not identical with the opposition between left and right (Merle, 1969) and that antiracist and colonial attitudes can very well be combined (Bolt, 1971). Forster's un impeachable insight into the close connection between science (of man) and politics consists precisely in the fact that he upholds the separation of science and politics and perceives that 'practical education' is more suitable for bringing humanitarian ideals closer to realization than scientific 'blind faith'–even if that faith is propogated with a progressive intention.

The appropriateness of placing Forster's dispute with Kant in this perspective of the relation between science and politics rather than in a narrow reference to the race theme is shown by a reading of the Forsterian epistolary. The most informative is the letter that Forster wrote to Heyne on January 21, 1787, from Vilna: 'I found it a great joy that you liked my article in the *Merkur*. Apart from the fact that Herr Kant here really leads us around in a circle and assented to find a concept that he had already given in the hypothesis, I believe that it can do no harm to view things sometimes from the other side. It is indeed still possible that all men derive from one single pair only it does not lend itself to proof by the kind of attempts made up to now. And what must be most reassuring is that in the end it is a matter of great indifference, and must be so even to the theologian, when he does not cling to prejudices from which the better men of this estate have long withdrawn' (VII, pp. 370–371).

REFERENCES

Ackerknecht, Erwin H. (1954), On the comparative method in anthropology, In Robert F. Spencer (Ed.), *Method and perspective in anthropology. Papers in honor of Wilson D. Wallis.* Pp. 117–125.

— (1955), George Forster, Alexander von Humboldt, and ethnology, *ISIS,* 46: 83–95.

Aron, Raymond (1970), Le Paradoxe du même et de l'autre, *Echanges et Communications. Mélanges offerts à Claude Lévi-Strauss à l'occasion de son 60ième anniversaire, réunis par Jean Pouillon et Pierre Maranda,* vol. 2. The Hague, Paris, Mouton, Pp. 943–952.

Bastian, Adolf (1900), Anthropologie und Ethnologie, *Die Völkerkunde und der Völkerverkehr unter seiner Rückwirkung auf die Volksgeschichte.* Berlin, Weidmannsche Buchhandlung. Pp. 1–11.

Bilz, Rudolf (1971), *Paläoanthropologie. Der neue Mensch in der Sicht der Verhaltensforschung.* Frankfurt am Main, Suhrkamp.

Blome, Hermann (1939), Der Gestaltwandel der Rassentheorie Kants in der deutschen Romantik, In Hans Plischke (Ed.), *Göttinger völkerkundliche Studien.* Leipzig, Otto Harrassowitz. Pp. 11–44.

— (1943), *Der Rassengedanke in der deutschen Romantik und seine Grundlagen im 18. Jahrhundert.* Munich–Berlin, J. F. Lehmanns Verlag.

Bodi, Leslie (1959), Georg Forster: The 'Pacific expert' of eighteenth-century Germany, *Historical Studies: Australia and New Zealand,* 8 (32): 345–363.

Bolt, Christine (1971), *Victorian attitudes to race.* London, Routledge & Kegan Paul.

Cunningham, D. J. (1908), Anthropology in the eighteenth century, *Journal of the Royal Anthropological Institute,* 38: 10–35. London.

Fischer, Hans (1970), 'Völkerkunde', 'Ethnographie', 'Ethnologie'. Kritische Kontrolle der frühesten Belege, *Zeitschrift fur Ethnologie* 95 (2): 169–182.

Forster, Georg (1843), *Sämmtliche Schriften.* Herausgegeben von dessen Tochter und begleitet mit einer Charakteristik Forster's von G. G. Gervinus. Leipzig, E. A. Brockhaus.

— (1958), *Philosophische Schriften.* Mit Einführung und Erläuterungen herausgegeben von Gerhard Steiner. Berlin (GDR), Akademie-Verlag.

— (1968), *Werke in zwei Bänden.* Berlin (GDR) und Weimar, Aufbau-Verlag.

— (o. J.), *Werke in vier Bänden.* Herausgegeben von Gerhard Steiner. Frankfurt am Main, Insel.

— (1914), *Tagebücher.* Herausgegeben von Paul Zincke und Albert Leitzmann. Berlin, B. Behr's Verlag.

Forster, Johann Reinhold (1787), *Bemerkungen über Gegenstände der physischen Erdbeschreibung, Naturgeschichte und sittlichen Philosophie auf seiner Reise um die Welt gesammelt.* Übersetzt und mit Anmerkungen vermehrt

von dessen Sohn und Reisegefährten Georg Forster ... Wien, gedruckt bei Johann Thomas Edlen von Trattnern.

Foucault, Michel (1966), *Les mots et les choses. Une archéologie des sciences humaines.* Paris, Editions Gallimard.

Frölich, Carl Wilhelm (1972), *Über den Menschen und seine Verhältnisse.* Berlin, Frankesche Buchhandlung.

— (1960), *Neudruck.* Herausgegeben und eingeleitet von Gerhard Steiner. Berlin (GDR), Akademie-Verlag.

Gehlen, Arnold (1961), *Anthropologische Forschung. Zur Selbstbegegnung und Selbstentdeckung des Menschen.* Reinbek b. Hamburg, Rowohlt.

— (1966), *Der Mensch. Seine Natur und seine Stellung in der Welt.* Frankfurt am Main/Bonn, Athenäum.

Goldschmidt, Walter (1972), An ethnography of encounters: A methodology for the enquiry into the relation between the individual and society, *Current Anthropology* 13 (1): 59–78.

Harris, Marvin (1968), *The rise of anthropological theory: A history of theories of culture.* London, Routledge & Kegan Paul.

Hegel, Georg Wilhelm Friedrich (1970), Enzyklopädie der philosophischen Wissenschaften III. In Eva Moldenhauer and Karl Markus Michel (Eds.), *Theorie-Werkausgabe,* Band. 10. Frankfurt am Main, Suhrkamp.

Heine-Geldern, Robert (1964), One hundred years of ethnological theory in the German-speaking countries: Some milestones, *Current Anthropology* 5: 407–418.

Heinemann, Fritz, Ed. (1929), *Wilhelm v. Humboldts philosophische Anthropologie und Theorie der Menschenkenntnis.* Halle/Saale, Max Niemeyer.

Herder, Johann Gottfried (1967), Ideen zur Philosophie der Geschichte der Menschheit. Erster und zweiter Teil (1784/85). In Suphan (Ed.), *Sämtliche Werke,* Band XIII. Hildesheim. Georg Olms Verlagsbuchhandlung, Nachdruck.

Hinske, Norbert (1966), Kants Idee der Anthropologie, *Die Frage nach dem Menschen. Aufriss einer philosophischen Anthropologie.* Freiburg–Munich, Karl Alber. Pp. 410–427.

Horkheimer, Max and Adorno, Theodor W. (1944), *Dialektik der Aufklärung. Philosophische Fragmente.* Amsterdam, Querido.

Hubert, René (1923), *Les sciences sociales dans l'Encyclopédie. La philosophie de l'histoire et le problème des origines sociales.* Paris, Félix Alcan.

Ihle, Alexander (1931), *Christoph Meiners und die Völkerkunde.* Göttingen, Vandenhoeck & Ruprecht.

Jettmar, Karl (1964), Fruchtbare Ansätze im ethnologischen Denken der Vergangenheit, *Festschrift für A. E. Jensen.* Munich, Klaus Renner. Pp. 267–277.

Kant, Immanuel (1968), *Werke in zehn Banden.* Weischedel (Ed.). Darmstadt, Wissenschaftliche Buchgesellschaft.

Krader, Lawrence (1968), "Anthropological traditions: Their relationship as a dialectic" (see pp. 000, this volume).

— (1972), *The ethonological note books of Karl Marx*, Assen, Van Gorcum & Co.

Landmann, Michael (1962), *De Homine. Der Mensch im Spiegel seines Gedankens.* Freiburg/Munich, Karl Albert.

Lepenies, Wolf (1971), *Soziologische Anthropologie. Materialien.* Munich, Carl Hanser.

Lichtenberg, Georg Christoph (1921), *Briefe an Johann Friedrich Blumenbach*• Albert Leitzmann (Ed.). Leipzig, Dietrichsche Verlagsbuchhandlung.

— (1967ff.), *Schriften und Briefe in vier Bänden.* Munich, Carl Hanser.

Lowie, Robert H. (1928), Individual differences and primitive culture, In W. Koppers (Ed.), *Festschrift für P. W. Schmidt.* Pp. 495–500.

— (1937), *The History of Ethnological Theory.* New York, Farrar & Rineharti Inc.

Luhmann, Niklas (1970), *Soziologische Aufklärung. Aufsätze zur Theorie sozialer Systeme.* Cologne and Opladen, Westdeutscher Verlag.

Lukács, Georg (1954), *Der junge Hegel und die Probleme der kapitalistischen Gesellschaft.* Berlin (GDR), Aufbau-Verlag.

Mannheim, Karl (1965), *Ideologie und Utopie,* Louis Wirth and Edward Shils (trans.). New York, Harcourt, Brace and World.

Marquard, Odo (1965), Zur Geschichte des philosophischen Begriffs 'Anthropologie' seit dem Ende des achtzehnten Jahrhunderts, *Collegium Philosophicum. Studien Joachim Ritter zum 60. Geburtstag.* Basel and Stuttgart, Schwabe & Co. Pp. 209–239.

— (1971), Anthropologie, In Joachim Ritter (Ed.), *Historisches Wörterbuch der Philosophie,* Band 1. Basel and Stuttgart, Schwabe & Co.

Merle, Marcel (1969), *L'anticolonialisme européen de Las Casas à Marx.* Paris, Armand Colin.

Merton, Robert King (1966), *Social theory and social structure.* New York, The Free Press.

Moleschott, Jacob (1857), *Georg Forster, der Naturforscher des Volkes.* Frankfurt am Main, Verlag von Meidinger Sohn & Cie.

Mühlmann, Wilhelm Emil (1955), *Arioi und Mamaia. Eine ethnologische, religionssoziologische und historische Studie über polynesische Kultbünde.* Wiesbaden, Franz Steiner Verlag.

— (1966), Umrisse und Probleme einer Kulturanthropologie. In W. E. Mühlmann and E. Müller (Eds.) *Kulturanthropologie.* Cologne/Berlin, Kiepenheuer & Witsch.

— (1968), *Geschichte der Anthropologie.* Frankfurt am Main/Bonn, Athenäum.

Plessner, Helmuth (1965), *Die Stufen des Organischen und der Mensch.* Berlin, Walter de Gruyter & Co.

Plischke, Hans (1931), *Die ethnographische Sammlung der Universität Göttingen, ihre Geschichte und ihre Bedeutung.* Göttingen, Vandenhoeck & Ruprecht.

— (1937), *Johann Friedrich Blumenbachs Einfluss auf die Entdeckungsreisenden seiner Zeit.* Göttingen, Vandenhoeck & Ruprecht.

— (1960), Insulaner aus der Südsee in Europa am Ende des 18. Jahrhunderts, *Ethnologica. Neue Folge,* Band 2. *Völkerkundliche Forschungen.* Cologne, E. J. Brill. Pp. 94–104.

Poirier, Jean (1968), Histoire de la pensée ethnologique, *Ethnologie Génerale.* Paris, Encyclopédie de la Pléiade.

Pouillon, Jean (1971), Traditions in French anthropology, *Social Research,* 38 (1): 73–92.

Raynal, Guillaume-Thomas, (1781), *Histoire philosophique et politique des établissements et du commerce des européens dans les deux indes.* Geneva, Jean-Leonard Peller.

Rödel, Wolfgang (1960), *Forster und Lichtenberg. Ein Beitrag zum Problem deutsche Intelligenz und Französische Revolution.* Berlin (GDR), Rütten & Loening.

Slotkin, J. S., Ed. (1965), *Readings in early anthropology.* London, Methuen & Co.

Smith, Bernard (1960), *European vision and the South Pacific 1786–1850: A study in the history of art and ideas.* London, Oxford University Press.

Schmidt, P. W. (1906), Die moderne Ethnologie, *Anthropos* 1: 134–163, 318–387, 592–643, 950–997.

Stocking, George W., Jr. (1968), *Race, culture, and evolution: Essays in the history of anthropology.* New York, The Free Press.

Tax, Sol (1955), The integration of anthropology, *Yearbook of anthropology.* New York, Wenner-Gren Foundation for Anthropological Research. Pp. 313–329.

Thompson, Laura (1967), Steps toward a unified anthropology, *Current Anthropology,* 8 (1/2): 67–77.

Träger, Claus (1962), Georg Forster und die Verwirklichung der Philosophie, *Sinn und Form,* 14: 625–649.

Uhlig, Ludwig (1965), *Georg Forster. Einheit und Mannigfaltigkeit in seiner geistigen Welt.* Tübingen, Max Niemeyer.

"Anthropological Traditions: Their Uses and Misuses" (see pp. 000, this volume).

19 Yugoslavia: The practical critique of ideological Marxism

BOGDAN DENITCH

Introduction

Even a cursory glance at the growing literature on social science in Eastern Europe will indicate that anthropology, as it is understood in the West, is the least developed of the social sciences. A recent issue of the *East European Quarterly* (4 [3], Sept. 1970) on anthropology in East Central and Southeast Europe consists exclusively of articles on ethnography, ethnology and archaeology. The only reference to anthropology is a short contribution on cultural anthropology in Rumania which is primarily an article on ethnology.

In an article on the development of sociology in Eastern Europe, I have argued that from the point of view of professional opportunity, sociology and social science were most developed in Poland and Yugoslavia (Denitch, 1971). Development in this case refers to the range of problems to which a local scientist can address himself and the flexibility available in picking the instruments and framework of research. Therefore, an examination of the situation of anthropology in Yugoslavia is relevant to Eastern Europe; if anthropology were to develop in Eastern Europe, it would develop first in Yugoslavia.

For almost two decades Yugoslav social scientists have been exposed to continual contact with their Western colleagues, including American anthropologists doing field work in Yugoslavia. A voluminous bibliographical article by Joel Halpern (1970) covers much of the recent Anglo-American work in the field. To be sure, the work of most of the Anglo-American anthropologists is closer to that of the ethno-

graphers. The major exceptions, Eugene Hammel (1969b, 1969c)[1] and Bette Denitch (1968, 1969) found it necessary to work with sociologists, since their basic interest in cultural and social change was shared by sociologists and social psychologists rather than by ethnographers.

Ethnographers, as guardians of traditional culture and traditional values in modernizing societies, are worthy of study in themselves. It is no accident that most ethnologists and ethnographers in Yugoslavia tend to be traditionalists (working outside of the Marxist tradition) who stress that which is unique and specific to their societies rather than general themes of interest to other developing societies.

Such scholars, particularly in Yugoslavia, can most usefully be thought of as students of folklore and folk life. By reviving an interest in the folk (most people in Eastern Europe were peasants) they played an indispensable, formative role in stimulating national consciousness among hitherto dormant nationalities. They were thus counterposed against the major intellectual currents of the nineteenth century, which stressed universalism and a model of industrial society in which nation and nationality were to be understood as coterminant with the boundaries of the existing states. In Eastern Europe, these states were multinational, although they did not recognize the nonruling nations as having national rights. At most, they granted a degree of linguistic autonomy to minority subjects. In Austria-Hungary, for example, revived Magyar nationalists, while insisting on their cultural and historic rights, denied the latter to the 'ahistorical peoples' of Hungary who were, in fact, compact majorities in their own regions. Ethnologists, by insisting on the cultural uniqueness of these peoples, rooted the new nationalism in popular usage rather than in the framework of political legitimism which identified nationality with the nation-states, the nobility and dynasties. In that sense, traditional ethnography had a profoundly democratizing impact on these polities.

In Yugoslavia the term anthropology has been used in a number of ways, indicating the limits and the direction of current development. Initially, as adopted primarily by scholars in Slovenia, it referred exclusively to physical anthropology. Since World War II, Vera Ehrlich (1966) has published a major study of 300 villages, which appeared both in Serbo-Croatian and in English and which resembles much of the

1. These examples of Hammel's work are similar to the work of Yugoslav *sociologists.*

work done by American anthropologists on microsystems in the period before World War II. As a member of the Faculty of Philosophy at the University of Zagreb, she also teaches cultural anthropology.

More recently, the term anthropology has been used by two philosophers, Vanja Sutlic and Zagorka Pesic-Golubovic, of Zagreb and Belgrade, respectively, who are members of what can be broadly defined as the humanist Marxist tradition. Pesic-Golubovic has written a monograph, *Predmet Socijalne Antropologije (The subject of anthropology)* (1968), which summarizes the particular approach of that nascent school, which has no real equivalent in American anthropology. Broadly speaking, it is the consideration of man's place taken evolutionarily, in the universe, and at least in Pesic-Golubovic's writings, it acts as the link between philosophy and social science. Manojlo Gluscevic has written several articles on anthropology, as well as a seminal article on anthropology and sociology (1964) which basically introduces the work of Western anthropologists to the Yugoslav social sciences. He and some of the younger researchers, focusing on rural sociology, are the closest equivalents of their Western colleagues today. It should be noted, however, that their interests are peripheral to the main thrust of current Yugoslav sociology.

Yugoslav sociology is, above all, concerned with the urban modern sector and with institutions created after the Yugoslav revolution, particularly the workers' councils. To be sure, Rudi Supek, one of the doyens of Yugoslav sociology, has been interested in cultural anthropology, and much of his work could equally well appear in journals of either anthropology or sociology. Supek, however, is an exceptional scholar whose interests range from philosophy, psychology and social psychology to political science, art and literature.

1. Why no anthropology: The role of ethnology

We may, therefore, ask why anthropology, as defined in Western Europe and the United States, is so underdeveloped in Yugoslavia and what the prospects for the field are in the near future. What should be stressed is that in the case of the Yugoslavs, it is not a lack of familiarity with the anthropological literature; rather, it is the current state of sociology, much the dominant social science, which determines the special situation of anthropology. The academic development of anthropology has

been blocked by the traditional organization of the faculties in which a chair of anthropology could be established only if the older and established ethnologists chose to move in that direction. Ethnologists in Yugoslavia, however, regard anthropology as a nonscience, as an adaptation of sociological and social psychological instruments to their field. They are concerned above all with studies of material culture, folk customs, peasant social organization and migrations. The studies of peasant social organization, however, are not perceived dynamically as an interaction between a traditional rural society and a modernizing Yugoslavia but rather as an attempt to seek the 'pure', original, unspoiled village culture. In that sense, they continue the tradition of nineteenth-century ethnology, which represented an effort at nation-building by defining the character of previously submerged nationalities.

Balkan cities had for centuries been exposed to the cosmopolitan pressures of the rival empires contesting that area and were often inhabited by aliens–Turks, Germans, Greeks, Armenians, Jews–until the mid-nineteenth century. The city was therefore regarded as an alien intrusion into the fabric of a national peasant society rather than a subject for a national ethnography. But more recent studies have indicated that the villagers themselves had changed significantly over time and had been deeply influenced by the various waves of conquerors. Thus, a more modern ethnology cannot help but trace the influence of Turkish, Greek, Venetian, German, Hungarian and other nationalities on the folk customs of the peasants and even on their social organization. The two institutions considered most specific to South Slavs in the Balkans, the *Zadruga* and the extended family, were at least revitalized by the Turkish and Austrian regimes, and their persistence in the nineteenth century is the result of the conscious policies of the ruling powers, particularly the taxation policy of the Turks, rather than of the 'national character'[2] in its supposedly original form.

Traditional ethnographers joined historians in the latter part of the nineteenth century in seeking to derive ideal types of national character from their studies. Unfortunately, this proved to be a field exploitable by chauvinist scholars; the Balkans, and particularly the South Slav areas of the Balkans are a mosaic of social organizations, and almost any kind of national character can be derived, provided one focuses

2. The most cogent discussion I have found of the Zadruga is by E. A. Hammel, 'Zadruga as a process' (mimeo); more accessible is Hammel (1969a).

on the ideal type desired. Thus, the Croat emigre scholar Dinko Tomasic, writing in 1948, reduced the major social tensions in Yugoslavia to the conflict between the aggressive, authoritarian, militaristic mountaineers from the Dinaric areas and the peace-loving, cooperative, democratic peasants from the plains. His thesis was that the bureaucracies and the military were rooted in the extended families of the Dinaric mountaineers and that they utilized their superior quasimilitary organization to repress the 'naturally' democratic peasants.

To round out this picture, he argued that the Dinaric mountaineers not only controlled the pre-World War I states of Montenegro and Serbia, and thus the interwar Yugoslav regimes, but were also the hard core of the new communist elite of Yugoslavia. Not coincidentally, he claimed that the Dinarics dominated Serbian social and political life, whereas the democratic peasants represented Croatia. The argument had the virtue of making the Serb mountaineers responsible for not only the prewar conservative exploitative bureaucracy but also the postwar communist regime. Tomasic's Serbian counterparts, particularly Jovan Cvijic,[3] argued along similar lines, although he reversed the assessment of the Dinarics and the plains folk. Thus, Cvijic defined the Dinarics as naturally democratic, state-building people who were indispensable to the creation of a modern Yugoslav state, whereas he defined the Panonian peasants (incidentally Croat) as atomized, selfish, unreliable and, above all, incapable of establishing a nation state.

Modern ethnography in Yugoslavia is a direct linear descendent of the prewar schools, and although the present political regime actively discourages the more virulent nationalist interpretations, the findings as well as the concern tend to follow lines established by Cvijic and his colleagues. This creates an inevitable tension among ethnographers, ethnologists and modern social scientists in Yugoslavia.

An ethnography which seeks to define the nation and national character in terms of its peasant past clashes with the values of modernity which are universally accepted by contemporary social scientists in Yugoslavia. A social science which concerns itself with the maintenance of 'tradition' clashes even more sharply with the needs of a modern multinational polity, since that tradition all too often was used politically to distinguish Moslems from Christians, Catholics from Orthodox, peo-

3. Cvijic's output was enormous. However, his most important work is Cvijic (1968).

ples from one region from those of another. In short, current ethnography in Yugoslavia is deified when it is either concerned with the remnants of an increasingly urbanized peasant culture or seeks to get beyond folklorist work or studies of material culture. In either case it provides a rationalization for chauvinistic nationalism.

Despite the conservative influence that the current departments of ethnology and ethnography have on the development of Yugoslav social science, their historic role must be seen in perspective. They were, after all, the intellectual underpinning of the early populist nationalism in Eastern Europe, counterposed against the older schools which defined the nation as the political nation, that is, as the gentry and the other strata which were the carriers of the entrenched upper-class culture. By insisting on the fundamentally popular and peasant base of the new nationalism, however, ethnologists helped awaken the dormant peasantry in the nineteenth century. It was only after Yugoslavia gained formal independence after World War I that the ethnologists' role shifted and became more conservative. In part this is because, supposedly following of Marx and Engels, workers' movements all too often tended to be grossly intensitive to the problem of national identity, particularly when that identity involved 'ahistorical minor peoples'. Marxist parties existing before the formation of the Communist International saw the fate of the smaller nationalities as a merging with larger political-economic units. The Austrian-Social-Democratic movement was indeed sensitive to the individual rights of the workers of the non-German and Hungarian nationalities but regarded their national aspirations as retrogressive and conservative. The ethnologists became spokesmen for the more traditionalist and conservative nationalist elements which based themselves ideologically on an idealized and increasingly inaccurate picture of village and peasant life, which in turn was supposed to produce a political base for more traditional notables who were to be the repositories of real, organic nationalism.

In order for modern anthropology to develop out of ethnography, the present school of ethnographers would have to be replaced by a younger generation concerned with Yugoslav problems in a comparative context and in relation to the dynamic modern strata. This is a problem familiar to American anthropologists, who for years have been accused of knowing more about obscure peoples in the Andes or the Western Pacific than about the ghettoes of Oakland and Harlem.

A still more cogent obstacle to the development of anthropology as

a discipline in Yugoslavia, and by inference in the rest of Eastern Europe, is the parochialism and provincialism of the social scientists themselves. This is ironic when one considers that Yugoslav sociologists have had extensive contacts with colleagues abroad. It can, however, be best understood by their continual stress on the uniqueness of the Yugoslav experience. This uniqueness is a *political* reality which is then extended to underlying social phenomena that are not at all unique. The Yugoslav socio-political experiment of self-management within a multinational polity has been stressed as representing a *unique* road to socialism; however, since the Yugoslavs had shied away from proposing their path as a general solution for other societies moving toward socialism, the tendency has been to study the effects of social change in Yugoslavia in isolation from analogous phenomena abroad. Morever, the intellectual atmosphere of the nineteenth century idealized nation-building and was thus particularly unsuited to viewing national development in a broad, comparative perspective. For example, Serbia in the nineteenth century was a small, underdeveloped state in a backwater of Europe and could not be considered particularly attractive as a model. Croatia and Slovenia were relatively underdeveloped provinces of the Austro-Hungarian Empire. Any comparison between the Yugoslav lands and the metropolises of the Empire would probably have had a discouraging effect on the growth of nationalism. Above all, a stress on singularity was necessary, or was viewed as such, in order to develop a national intelligentsia which would not be an echo of Vienna or Paris culturally but would relate to the problems of the broad peasant masses of the nation. There, too, an analogy can be found in the American experience, in the recent work of Black nationalist scholars who, for the purpose of developing national pride and identity, place an overwhelming stress on that which is unique and specific to the Black experience rather than on the equally important common experience of the Blacks with other ethnic groups in the United States.

Most Eastern European countries went through the nation-building phase relatively late in their history, and the legitimacy of their national existence is repeatedly emphasized and reemphasized by historians, writers and social scientists.[4] This resistance to considering general

4. Even a cursory glance at the recent social science and literary journals in Yugoslavia shows a preoccupation with the 'national question'; the last two years have witnessed a revival of nationalist historiography, with a stress on the nineteenth-century nation-building period.

phenomena in their societies can be expected to continue as long as national integration within the framework of a new socialist polity does not become sufficiently firm for it to be taken for granted. I remember, for example, when stressing certain comparative aspects of Yugoslav urbanization with that of Mexico, the shocked reaction of my colleagues; after all, Mexico was that strange, exotic and underdeveloped land, and Serbian peasants could hardly be compared to their Latin American counterparts. Even Yugoslav economists repeatedly run into the problem of what is a valid comparative base for the Yugoslav performance. Branko Horvat was received with bewilderment when he stressed that Yugoslav development ought to be compared with parallel processes in Portugal, Turkey and Greece.[5] The point is that the frame of reference of most Yugoslav intellectuals is not shaped by the material realities of their society, which is underdeveloped, but rather by the Western industrialized countries.

It is also clear that the Western form of anthropology developed when the ruling establishment required knowledge of other peoples. It is surely no accident that modern anthropology emerged in countries with subject and colonial populations, whose customs and social organization were of immediate practical interest to the colonial proconsuls and administrators. One could argue that anthropology developed in the Soviet Union for that same reason. In Russia, however, there are ideological obstacles to the development of any social science that would position the Soviet system in a potentially critical, and therefore changeable, comparative framework. The very introduction of a comparative approach implies that one's own society is not necessarily a fixed model, that there are different and equally valid ways of organizing social tasks and that, presumably, this may, theoretically, influence the society from which the scholar comes. The Eastern Europeans had neither the need to study other dependent peoples nor such a well-integrated sociopolitical system that studies treating their own society comparatively would be encouraged. In Yugoslavia the existence of a state based on nations at different levels of development but sharing the same ideology and political organization argues for a comparative approach. However, I believe that the trend toward comparative research, often handling themes common to Western anthropologists, will develop within the discipline of sociology.

5. Personal observation at Praxis Summer School, August 1971.

Yugoslav sociology finds itself today sharply divided between two extremes. On one hand, the heritage of philosophy and Marxism produces a great deal of theoretical work, as often as not unlinked to any empirical research. Sociology is often still a surrogate for 'the science of society'. It is therefore a more modern and scientific way of teaching Marxism in secondary schools and, to a certain extent, in the universities. That at least is the content of the elementary courses of sociology. On the other hand, empirical research has developed rapidly through a network of institutes and study centers, often unnecessarily duplicating the work of the others and engaged in operational research.[6] There we are faced with barefoot empiricists who often seem to imitate the worst aspects of their American counterparts. Endless survey studies have been undertaken for almost two decades; however, this has been a necessary stage in freeing the discipline from its dogmatic Marxist past. It is only now that a new generation of sociologists *trained as sociologists* is emerging from the universities and is once again turning to a search for meaning and more theoretical constructs.

Thus we can see the development of a new Marxist sociology in Yugoslavia, using empirical research as a tool and not as an end, and seeking to tackle broader social problems within Yugoslav society. Since the central problems of Yugoslavia are those of a developing society with a rapidly changing nexus of values and behavior, the need for social anthropologists becomes more and more evident. The most neglected area of research in Yugoslavia has been the rural sector. There is a great deal of resistance epistemologically among urban intellectuals to recognizing the importance of a sector associated with all that is 'backward' and traditional. However, the growing attention to the consequences of social change in Yugoslavia will lead more researchers to deal with the stresses and strains in their society, and that will certainly encourage them to develop a framework in which they can examine that which is unique and generally relevant in contemporary Yugoslav experience.

6. *Yugoslav research guide* (English edition) lists the numerous institutes in Yugoslavia. Given the stress on the republics' autonomy, the major institutes are duplicated in most republics. Thus there are *major* centers in Belgrade, Zagreb, Ljubljana, Sarajevo and Skopljc.

2. Comparative relevance of social conflicts in Yugoslavia

Yugoslavia has often been desribed as an almost ideal laboratory for political sociologists and social anthropologists. It is as relevant to those who are primarily interested in studying historic problems of development – the transition from the prerevolutionary Yugoslav state to later socialist Yugoslavia offers almost endless parallels to scholars concerned with the problem of nation-building – as it is for those primarily concerned with contemporary society. It is therefore unfortunate that Yugoslav scholars themselves have not, with minor exceptions, used comparative methods in their voluminous studies on pre- and postrevolutionary Yugoslavia. The work of foreign scholars, primarily American, has been deeply imbued by the positivist and functionalist orientations of American social scientists or it has been an application of various theories developed in the context of the cold war.

Prerevolutionary social scientists, historians and ethnologists in the Yugoslav lands had developed primarily under the influence of various strains of nationalism in counterposition to the older legitimist schools. Both major schools tended to stress the uniqueness of the national mission of the particular nation rather than those factors which were universal and had a relevance to other polities. Thus, much of prerevolutionary ethnography suffered from intense chauvinism when it was not influenced by the fashionable racism of the late nineteenth century. A good example can be seen in the work of the Serbian ethnographer already mentioned, Jovan Cvijic, whose studies coincided with the aspirations of the Serbian bourgeoisie to become dominant in the Balkans. His repeated stress on the unique 'state-building' characteristics and special mission of the Dinaric mountaineers paralleled the social mythology of the military and political cliques dominating Serbia and Montenegro. Cvijic's 'scientific' ethnographic maps of Macedonia and southern Serbia, where the borders of Serbian nationality shifted from year to year, simply matched the increasing claims of the Serbian government in that area. Historians tended to place enormous emphasis on that particular period in the history of the given nation which coincided with its greatest geographic expansion. Thus, much of the nineteenth-century history was simply used to create a basis for legitimist-historical claims for territorial expansion and dominance. This was as true of the historians working in the independent states of Serbia and Montenegro as it was of the south Slav historians within the Austro-

Hungarian Empire. The latter developed a school of history primarily antagonistic to the claims of the Hungarians and Austrians and attempted to create, sometimes by outright forgery, a historico-legal claim of national continuity within expanded frontiers.

Not only was the intelligentsia of the Yugoslav lands dominated by conflicting nationalist sentiments and claims, but major institutions, such as the Church, also sanctioned such claims. It would be almost impossible to isolate the Serbo-Croatian conflicts under the Austro-Hungarian monarchy, and even in modern Yugoslavia, from the respective roles of the Catholic and Orthodox Churches. As is well known, the genesis of Bosnian Moslem Slav identity is to be found in Islam. Thus, one of the first tasks for a comparative sociologist, political scientist or anthropologist is to cut through the underbrush of chauvinist propaganda, disguised as scholarly work, and to address himself to the problem of *what forces existed*, both externally and internally to Yugoslav society, which *did* lead to the establishment of a Yugoslav state in 1918. With the decline of colonialism, the new nations often included diverse peoples and rival religious establishments. Therefore, such a study would be of general relevance, for example, in Africa.

Since two separate attempts to establish the basis for a Yugoslav community have taken place – one, under the national bourgeoisies between 1918 and 1941; the other, following the Yugoslav revolution of 1941-1945-we are presented with alternative approaches to nation-building. The advantage for comparative study is furthered augmented by the fact that the Yugoslav revolution went through clearly differentiated stages, and the record of success and failure has great comparative relevance.

Since Yugoslavia has three major religious denominations, whose respective adaptability to a modern socialist state can be examined, contemporary Yugoslavia permits us to study the comparative resilience of traditional institutions, such as religion, while controlling for such factors as the level of development, literacy and national tradition.[7] Yugoslavia today displays a range of development which could be summarized as revealing characteristics from Central Europe to Central Asia. Thus, the impact of *conscious national policies* on such factors as social mobility, education, natality and urbanization can be examined

7. There *has* been a developing interest in the study of sociology of religion in Yugoslavia. See, e.g., Bosnjak (1969), Cimic (1969), Bahtijarevic (1969) and Bogdan Denitch (no date).

over a range of backgrounds. It also means that Yugoslavia, willy-nilly, is a possible model both for countries in the middle range of development and for countries designated as underdeveloped.

The most significant single cleavage in Yugoslav society as a whole is the urban-rural one. This problem, central to all developing societies, is particularly sharp in Yugoslavia. One could almost talk of 'two nations', roughly equal in size but grossly unequal in income, influence and power. The peasantry is not only practically excluded from the most dynamic sectors of Yugoslav society and its institutions, but it is also subject to pressures which place it in an inferior position in the society as a whole. To begin with, the very structure of representation is biased against the peasant. The system of representation on the commune level grants the socialist sector an automatic half of the seats, and no specific organization analogous to the trade unions exists for the articulation of peasant interests as a whole. To be sure, representation is biased in the same way against the unemployed, the housewives and those in the private sector, but the very placing of the peasantry within that triad is unacceptable to the working peasants who have contributed heavily to the economic development of the country.[8] Culturally, the peasants are downgraded; the mass media portrays the only desirable existence as urban and modern and the peasantry as primarily an object of humor. The effect is that the young and ambitious are under continual pressure to abandon the villages, unfortunate at a time when the concomitant rapid expansion of urban housing and employment does not exist.

On the other hand, it is true that the peasantry has been more or less left to its own devices, no longer subject to various campaigns directed against private peasant property and village life. This also means that the sociopolitical organizations which would link the peasantry to the modern sector, such as the League of Communists, tend to atrophy in the villages, having little relation to the day-to-day problems of their constituencies. The growing decentralization of Yugoslav society and the widespread introduction of the market principle have meant that the relative development of cultural institutions in the villages has stagnated,

8. The major journal in Yugoslavia addressing itself to the problems of peasantry is *Sociologija Sela,* edited by Dr. Stipe Suvar in Zagreb. *Cf.* Halpern (1958) which, along with his numerous articles, remains the best source in English. For an historic background of the peasant problem in Yugoslavia, see Tomasevich (1955). The 1961 census figures give a breakdown by economic activity (of the *economically active*) as agricultural 51.01% and non-agricultural 39.01%.

if not retrogressed. This is evidenced by, among other things, the hard-core illiteracy as an aspect of the even wider problem of functional illiteracy in the villages. One could argue that a relatively underdeveloped society could not simultaneously engage in the tasks of creating an advanced urban industrial sector and modernizing the countryside. The fact does remain, however, that the Yugoslav *social* revolution is an urban, industrial phenomenon today. The long-range integration of the peasantry with the modern sector remains one of the major problems, and it is an indication of a continued antipeasant bias that it is a problem to which little attention has been paid by social scientists. Modern anthropologists could, of course, make a major contribution in this area.

Another cardinal problem of Yugoslav society is how to replace a revolutionary generation so that political continuity can be maintained. This is a problem central to many societies that have undergone a social revolution, but it is found in a particularly concentrated form in Yugoslavia. There are two factors involved: First, the relative homogeneity of the generation of war and revolution means that the men in power will tend to retire at the same time; second, their replacements will come from strata that could not have gone through any analogous experience. The homogeneity of the age cohort is derived from the brutal needs of guerrilla warfare during the war, which placed an accent on youth, creating a cadre, formed mostly during the years 1941 to 1945, which was unusually young when it came to power. The membership of the Yugoslav Community Party in[4] June 1945 was 140,000, *but only 3000 members of the prewar Party survived the war*. Thus, the revolution served as the apprenticeship for the leading cadre on all levels of society. That experience cannot be duplicated. The rising generation has been formed by the routinized path of *education, career* and *political service*. Moreover, this group has neither the homogeneity nor the solidarity of the previous generation. They act in the context of republics and localities rather than within a national arena. Therefore, the absence of the cross-cutting role played by the relatively unified revolutionary cadre can become a serious problem in the future.

The effects of decentralization reinforce the localist rather than the universal character of the new leadership, since political careers generally develop within the frontiers of a single republic. The experience of other multinational polities suggests the serious problem posed by the development of a fragmented national leadership within a society in which power accumulates along geographic lines.

The problem of multinationality provides further relevance for comparative studies of the Yugoslav experience. The relatively naive optimism of both the liberal nation-builders of the nineteenth century and the early workers' parties is belied by recent developments. *The belief that localism, particularism and nationalism were phenomena associated with traditional societies which would increasingly diminish as a process of modernization and industrialization occurred is contradicted by the growing phenomena of localism and particularism in industrial Europe itself.* The vitality of nationalism can be seen in the Catalan-Castilian dispute in Spain, the Walloon-Flemish conflict in Belgium, Slovak-Czech tensions in Czechoslovakia, the recent intranational struggle in Canada and the widening demand for regional autonomy even in relatively homogeneous states such as France, Italy and England.

It seems axiomatic that the search for national and local roots is a growing general reaction to the impersonalization and atomization of modern industrial society. The quest for identity apparently requires more intimate and personal symbols than those of the nation-state itself, and, in many cases, the only symbols on hand are the traditional ones. Of course, such identity can also be provided by mass political and social movements; however, this is not the case when *movements* become the *establishment. For that reason, nationalism as a phenomenon will not decline even in relatively stable developed societies, and attempts to resolve the need for national identity within the framework of a unified polity will become a widening concern.* In this respect, the Yugoslav experience proposes solutions, making it almost an alternative model for the advanced polities.

The Yugoslav approach could be defined as a counterthesis to the generally accepted Jacobin model of organization of the modern-state. This latter model assumes the identification of the citizen with the state and considers that the primary problem of representation is to assure some kind of fair interest aggregation on a geographic basis. It regards other factors, such as nationality, profession and the work place, as fundamentally illegitimate or archaic ways of fragmenting representation, since they are contrary to the concept of a "national" interest.

Clearly, the Yugoslav model,[9] which accepts the legitimacy of multi-

9. For a description of the Yugoslav 'model', see Vucinic (1969) and Zaninovich (1968). Both are already slightly outdated as a consequence of continual evolution

nationality, institutionalizes it in the organization of the state itself and provides for a multiplicity of national, regional and functional representation to the various chambers of the legislature, is an alternative approach geared to the fact that a modern citizen has a number of reference groups, any one of which at a given point can become the most relevant. Thus, a citizen can be at the same time (1) a member of a nation, (2) an adherent of a political party and (3) a member of an economic organization or profession, etc., and at any moment any of these identities may become more critical in reflecting his needs and demands than the others. In modern industrial society, probably one of the least relevant bases for representation is that of sheer geographic proximity.

Cultural and linguistic identity are scrupulously respected in the formal organization of the Yugoslav state. Each of the major nations–Slovenes, Serbs, Croats, Montenegrins, Macedonians and Bosnian Moslems–has a home republic, as do the two major minorities, Albanians and Hungarians, except that in their case this 'home' is in an autonomous province. The highest body in the legislature is based on an absolute parity of the republics, despite the fact that the population of Serbia is over eight million and that of Montenegro around one-half million. This body, the Chamber of Nationalities, can veto any legislation involving national rights passed by other chambers. Scrupulous attention is also paid to developing autonomous cultural institutions and educational centers, including universities. A cumbersome procedure has been established to provide as equal a representation in the Federal civil service as is possible, and in all republics considerable effort is devoted to cultivating national tradition, history and folklore.

Since other parliamentary bodies are based on functional representation as well as geographic representation, the term 'consociational' could be used to describe the formal Yugoslav model. Nevertheless, traditional national identity is threatened by industrialization, mobility and the development of a new set of values which are socialist and therefore not necessarily rooted in the locality. This tension between traditional nationalism and modern society in Yugoslavia has led to the develop-

---

and experimentation. Yugoslav sources are too numerous to cite; however, the two best sources in English are Horvat (1969) and the English editions of *Sociologia* (Belgrade, 1970) and *Ekonomist* (Zagreb, 1969). More recent developments can be followed in *Yugoslav Survey*, a quarterly (Belgrade), which summarizes the recent developments and laws from an official point of view. There is also a growing literature on the Yugoslav system of self-management.

ment of conservative, clerically oriented nationalist currents, most recently in Croatia, for which nationalism has become a surrogate for anticommunism. This is particularly obvious because the problems of national cultural identity have by and large been solved. Therefore, nationalism has to insist, in this case, on the type of traditionalist roots which are inconsistent with the present socioeconomic structure. This problem will undoubtedly remain acute until a new integration of national identity and modern industrial society is developed.

The more recent conflict in Yugoslavia over the development of the more backward areas is, of course, one problem common to most societies, even the most 'advanced'. It is a continual theme in Britain, France and, above all, countries like Italy, where wide gaps exist between the relative share of different regions in the gross national product. But in Yugoslavia, where representatives of the various republics are forced to hammer out agreements rather than resort to their respective abilities to mobilize their power on a statewide scale, a model for a solution perhaps exists. It is thus of great interest to many social scientists who believe that the mechanical aggregation of majorities is an increasingly less effective way of dealing with such problems. For example, the demands of minorities, such as the Blacks in the United States, are not resolvable by simple nose counts. The demands for cultural identity by the French of Quebec are not resolvable by an all-Canadian vote.

The wide interest in Yugoslav workers' councils and self-management is well known.[10] Some kind of participatory approach is now proposed in widely divergent economic and political institutions. Elements of self-management are even introduced in industry which remains under private ownership, and consultation has been generally proposed as one of the more effective mechanisms for modulating class and economic conflict. An obvious contrast exists between the view that self-management is *primarily* an instrument for increasing efficiency and *enterprise* loyalty and the one which views it as an instrument of class rule by the workers. In Yugoslavia the class themes are, of course, immensely

10. In 1966, the *Yugoslav* bibliography on self-management numbered 855 pages, making studies of self-management one of the fastest growing light industries in that country. A short summary in English can be found in *Yugoslav Survey*, 11 (August, 1970). *Cf.* Broekmeyer (1970). Two major Yugoslav works on self-management are Pusic (1968) and Zupanov (1969). A critical discussion can be found in Meiser (1967, 1971). Anthropos (Paris) also publishes a regular journal on the problems of self-management.

complicated by the natural tendency of the given economic unit toward a market-induced egoism. The differences between branches of the same industry, even when the skill level is kept constant, are greater than the income differences within the same enterprise at times. This particular line of inquiry – that is, into the balance between class consciousness and needs, projected in the general social and political arena, versus the 'vertical integrity' of structure that cuts across class lines – can be studied in Yugoslavia in a way not duplicatable in any other society.

The development of workers' self-management within the framework of a market economy has generated a whole set of real and potential social conflicts. These can be roughly divided between those occurring *within* the workplace and those between the workplace and institutions and forces outside of it. A workers' council is both the apex of an increasingly complex pyramid and a town meeting. An obvious conflict exists between the attempts by technocratically oriented managerial strata to stress efficiency and productivity and the natural resistance of the workers to any attempts to increase the pace of work or to alienate them from direct control over production. Increased availability of consumer goods and luxuries has created pressures by the professional strata for greater income differentials. Hiring university graduates from the outside rather than systematically upgrading workers, perhaps by sending them to universities, for professional jobs increases the gap between the 'experts' and blue-collar workers. And the tendency for some enterprises to merge makes the problem of representation ever more complex.

The conflicts between the enterprise and 'outside' forces are also systemic. The more obvious ones are those posed by the narrow self-interest of an enterprise–the scandalous pollution of rivers and parts of the seacoast, the production of goods whose social value is dubious, irresponsible pricing, the tendency to regard the enterprise as a form of collective private property, and the like. On the other hand, there are attempts 'from the outside' to interfere with the normal prerogatives of self-management in hiring and firing, demands for favors and the desire of poorer communities to tax the more successful enterprises for general development. It is clear from a number of studies that Yugoslav workers do have a higher level of participation and control than is the case in either the centralized socialist models or under capitalism. However, self-management cannot be treated in isolation from the rest of the society, and the study of conflicts within and outside

the boundaries of given enterprises will obviously have to relate to more general themes. Attempts to use a functional consensus model in studying the workers' councils which would treat conflict as dysfunctional to the stability of a system in equilibrium have no more in common with Marxist sociology than do similar attempts in industrial sociology in the West.

The absence of institutions which cut across geographic and social divisions creates a built-in factor stressing localism. Solutions are left to the automatic working of the economic, including market, forces, whereas in other societies they have required conscious intervention by parties and trade unions. The tendency to seek particularist roots in a modern industrial society would seem to me to lead almost inevitably to a revival of nationalism in its various forms unless conflict lines develop which cut across national divisions. The problem is familiar. For example, one publicized line of division within the left in the United States is the male-female conflict as symbolized by the women's liberation movement. However, cross-cutting that particular cleavage are at least two others which may be more relevant at a given moment–the Black-white cleavage and, of course, class divisions. In the absence of cross-cutting phenomena, the role of the women's movement would be to fragment Black and trade union organizations along sex lines.

By analogy, it seems to me that constructive social conflicts within a complex polity such as Yugoslavia would assure that cross-cutting cleavages nullify the tendency to a vertical aggregation of power within geographic units, a trend which is particularly dangerous since most geographic units are also *national* homes of one of the Yugoslav nations. *The cleavage lines would, therefore, serve to*[4]*bind the society together, since they permit alternative aggregations on different issues.* For example, the aggregation of interests from all Republics on the issue of economic development would unite their underdeveloped areas, cutting across formal Republican boundaries. On the issue of the social demands of the working class, a natural vehicle already exists in the trade unions, but they would have to act on a national scale in order to be effective. Such examples can, of course, be compounded.

A further conflict in Yugoslav society has been created by the rapid development of institutions of higher learning since World War II. Relatively underdeveloped, Yugoslavia has one of the highest proportions of university students to the given generation. Since there are no quota systems limiting the inflow of students in those faculties which do not

have a prospect of employment, an unemployed intelligentsia is created. The society as a whole pays for an expensive, often socially irrelevant, education primarily for the children of the better-off strata. The traditional value placed on nonmanual work gives the partially educated students a set of false aspirations which the society cannot fulfill since (hopefully) the number of white collar and bureaucratic jobs cannot expand indefinitely. Further, the rapid development of higher education has two negative consequences; on the one hand, it has led to a neglect of secondary and vocational education, and, on the other, it has introduced middle-class values and world views to an entire stratum of youth. *Status demands which run directly contrary to the egalitarian values of the revolution are therefore encouraged.*

A vast opening up of opportunities and personal horizons occurred during the course of the national liberation struggle and in the years immediately following. The patterns of traditional authority in the villages and towns had been shattered, but as the society became 'normal', traditional values tended to reassert themselves. Thus, the range of opportunities open to a young person today are more limited than they were immediately after the war. Generational cleavage is reinforced by the wholesale use of Western European models of behavior, pushed by the mass media, particularly the popular journals and television. The absence of conscious intervention by the sociopolitical activists, combined with a growing apoliticization, has probably led to the greatest erosion of socialist values. A question with which sociologists and anthropologists will certainly concern themselves in the near future is *to what extent does the socialization of a whole new generation reflect the effects of commercialized European culture* rather than the values of a socialist revolution? Further, to what extent are the new commercial values destroying the remnants of indigenous *national* cultures?

The extension of commercial criteria to culture has led to results which are nothing short of disastrous. The number of theaters in small towns throughout Yugoslavia has been declining for almost two decades. *Avant-garde* art forms are a copy of their equivalents in Western Europe. One cannot speak of any forms in any aspect of art today which are the products of a new, revolutionary society. Since art forms are, in part, a reflection of the social base and the values of a society, a critical area of social existence has no relationship whatsoever to the new models of social organization which exist in Yugoslavia. Under the guise of modernity, a recolonization of local culture has begun to take place.

Examining briefly the main conflicts in Yugoslav society as a whole, we can see that they apply to three types of societies: (1) Developing societies concerned primarily with nation-building and maintenance of national independence; (2) socialist societies facing the problems of decentralization; and (3) advanced industrial societies confronting the anomie and impersonalization generated by industrial development. In considering the range of problems, it is evident that conflicts in Yugoslavia are what one would expect to find in underdeveloped societies, although compounded by dilemmas now being confronted in the most developed ones. Therein one can find the basic contradiction in Yugoslav society, namely, that between the mundane possibilities of a relatively underdeveloped, small, independent nation-state and the daring effort to solve the complex problems of multinationality, industrial democracy, egalitarianism and social mobility in a way that has not yet been attempted anywhere else in the world. Therefore, the success or failure of Yugoslav social experiments is relevant to all societies seeking independent development and modernization; at the same time, it is an attempt, perhaps too advanced for the existing objective social and economic possibilities, to solve the problems facing socialism in advanced industrial societies.

Conclusion

Yugoslavia has been in the forefront in developing a *Marxist* sociology in Eastern Europe. The consciousness that Yugoslavia is an experimental society moving into a sphere of social relations where little or no experience exists as a guide and the growing openness which, I believe, remains the secular trend, despite recent set-backs, of Yugoslav Marxism argue that a modern Marxist anthropology will develop. In the immediate future, this will occur not on the boundaries of ethnology and sociology but rather as a synthesis of critical social philosophy and sociology. The reasons for this are historical and are to be found in the fact that the cutting edge of innovation in Yugoslav social science has come from the philosophers. The very definition of the subject of sociology by the philosophically oriented Yugoslav scholars is closer to the concerns of radical anthropologists than to the sociologists in the West. Rudi Supek (1971) states it as follows:

> With Marxism, sociology enters the essential dimension of human existence–the history of mankind. It is true that sociology can reveal, in the context of Max Scheler's question on 'man's position in the cosmos', the characteristics by which man differs from all other living creatures, as well as those he has retained from his animal origins. It can ascertain that human societies cannot be distinguished from animal ones by using the drives toward self-preservation and reproduction as the criterion of distinction, and that these drives are, in wars, ethnocentrisms, and nationalisms, more uncontrollable in man than they ever are in animals. It will also establish not only that these drives are more efficient in man but that the source of their efficiency lies in the power of assent and denial, affirmation and negation, *action* and *reflection*, by means of which man is a creative and a historical being – hence, a *being of praxis* and, as such, different from all animals. Man is capable of changing the conditions of his life, natural and social; but by changing the conditions of his life, man, as an 'objective being', simultaneously changes his very nature. That is why we speak of man as a creative and self-realizing being.

A precondition for the development of a sociology and anthropology within the Marxist tradition is the development of an open Marxism, one which utilizes critical tools of analysis in examining social reality. Unless the Marxist scholar is willing and able to view his own society, irrespective of its ideological claims, as being historically conditioned and subjected to the interplay of social forces and conflicts, his Marxism is reduced to an ideology. It is because Yugoslav critical Marxists do not view their own society as a finished system, but as a project – attempting to create a more human social order – that one can expect them to address themselves to the more universal issues of man's place in nature and society. To do this, they will have to retain and expand their critical independence *and* engagement. It is in this *praxis* that a genuine human social science can develop. What such a critical social science will be called is of secondary importance. Clearly, it will have to borrow from all social sciences; therefore, there is no reason why we should be expected to maintain the present artificial professional divisions among social psychology, sociology and social anthropology as institutionalized in the Western academy.

The empirical aspect of anthropology can be expected to come from the evolution of Yugoslav rural sociology, which is moving past the primarily descriptive phase and is confronting the problem of the integration of rural migrants into a new industrial working class. The character of the new working-class culture, in the socioeconomic context of self-management, has only begun to be examined. A second

problem which will increasingly draw attention is that of ethnic identity in a modern, socially mobile industrial society. In both of these areas, the comparative approach developed by social and cultural anthropologists is essential for any further advances. Both fields have been primarily descriptive, and, whereas this has provided the indispensable data for further development, the comparative dimension alone can provide a framework for analysis. Marxism was born as a philosophy and sociology of social change, and it did not become parochial until it was coopted and fossilized as the ideology of the ruling bureaucracy of the Soviet Union. The rebirth of Marxism as a relevant world view for social scientists must therefore involve a return to its critical and therefore comparative and engaged roots.

References

(I have generally limited myself to works available in English or French.)

Bahtijarevic, Stefica (1969), *Socialisticko Drustvo, Crkva i Religija.* Zagreb, Institute of Social Research.

Bosnjak, Branko, Ed. (1969), *Religija i Drustvo.* Zagreb.

Broekmeyer, M. J. (1970), *Yugoslav workers' self management.* Dordrecht, Holland, D. Reidel.

Cimic, Essad (1969), *Drustvo i Religija.* Belgrade.

Cvijic, Jovan (1968), *Balkansko Poluostivo.* Belgrade. (An older French edition was published in 1920.)

Denitch, Bette (1968), The strategy of mobility in Yugoslav industrialization. Paper presented at the meeting of the American Anthropological Association, Seattle, Washington.

— (1969), Social mobility and industrialization in a Yugoslav town. Doctoral dissertation, University of California, Berkeley. (To be published by the University of California Press.)

Denitch, Bogdan (1971), Sociology in Eastern Europe: Trends and prospects, *Slavic Review,* 30 (2), June.

— (no date), Religion and social change in Yugoslavia. Mimeo. (Available from BASR, Columbia University.)

Ehrlich, Vera (1966), *Family in transition: A study of 300 Yugoslav villages.* Princeton University Press.

Gouldner, Alvin W. (1970), *The coming crisis of Western sociology,* Chap. 12. Basic Books.

Gluscevic, Manojlo (1964), Antropologija i Sociologija, *Socioloski Pregled,* 1: 28–48. Belgrade.

Halpern, Joel M. (1958), *A Servian village.* New York, Columbia University Press.

— (1970a), A brief survey of English language research on Yugoslav cultural and social anthropology and ethnology. In Zdenck Salzman (Ed.), *A Symposium on East European ethnography* (Department of Anthropology Research Report No. 5). Amherst, University of Massachusetts.

— (1970b), Article, *East European Quarterly,* 4 (3): 320–342.

— and Hammel, E. A. (1969), Observations on the intellectual history of ethnology and other social sciences in Yugoslavia, *Comparative Studies in Society and History,* 11: 17–26.

Hammel, E. A. (1969a), 'Balkan' peasant: A view from Serbia. In Philip K. Brock (Ed.), *Peasants in the modern world.* Albuquerque, University of New Mexico Press.

— (1969b), Economic change, social mobility and kinship in Serbia, *Southwestern Journal of Anthropology,* 25: 188–197.

— (1969c), *The pink yo-yo: Occupational mobility in Belgrade 1915–1965* (Institute of International Studies Research Series No. 13). Berkeley, University of California Press.

Hofer, Tamas (1968), Anthropologists and native ethnographers in Central European villages in comparative notes on the professional personality of two disciplines, *Current Anthropology,* 9: 311–315.

Horecky, Paul, Ed. (1969a), *East-Central Europe: A guide to basic publications.* Chicago.

— (1969b), *South Eastern Europe: A guide to basic publications.* Chicago.

Horvat, Branko (1969), *An essay on Yugoslav society.* White Plains, N. Y., International Arts and Science Press.

Kanst, Roger E. (1971), *The behavioral revolution in communist studies.* Free Press. (Useful in discussing primarily the work of *Western* social scientists in Eastern Europe.)

Meiser, Albert (1967), *L'Autogestion yougoslave.* Paris, Anthropos.

— (1971), *Ou va l'autogestion yougoslave.* Paris, Anthropos.

Pesic-Golubovic, Zagorka (1968), *Predmet Socijalne Antropologije.* Belgrade, Institut Drustvenih Nauka.

Pusic, Eugen (1968), *Samouprauljanje.* Zagreb.

Supek, Rudi (1971), Sociology and Marxism, *International Journal of Sociology,* 1 (1).

Tomasic, Dinko (1948), *Personality and culture in East European politics.* New York, George Stewart.

Tomasevich, Jozo (1955), *Peasants, politics and economic change in Yugoslavia.* Standford.

Vucinic, Wayne S., Ed. (1969), *Contemporary Yugoslavia.* Berkeley, University of California Press.

Wiatr, Jerzy, Ed. (1971), *Sociology in Eastern Europe*. Indiana.
Zaninovich, George (1968), *The development of socialist Yugoslavia*. Baltimore, Johns Hopkins Press.
Zupanov, Josip (1969), *Samoupravejanje i drustvena Moc*. Zagreb.

Journals of general usefulness

L'Homme et la société. Paris, Anthropos.
Praxis (international edition). Zagreb.
Sociologia. (The basic Yugoslav journal, defined as a journal of sociology, social psychology and social anthropology which, although written in Serbo-Croatian, generally runs summaries in English.)
Survey. London.

20 The United States: The cultural predicament of the anthropologist

ERIC R. WOLF

Anthropologists typically pretend to be cultural communicants, in the two meanings relevant to the term (Barnhart *et al.*, 1958, p. 244), namely, one who partakes, or is entitled to partake, of the Eucharist; a member of a Church, and one who communicates. Since the term 'communication' itself has religious origins, referring to acts which bind those who partake together, there is an evident overlap between the two meanings. Indeed, it is possible to view anthropologists as religious communicants, building upon shared experience and belief and using that shared belief and experience to set themselves off from others who are not members of the same academic communion. In the European past, certain occupational groups were closely intertwined with religious service; and as anthropologists we have our own rites of transition, ranging from ordeal by examination to trial by field work, and our special rites of intensification when we rattle the bones of our high priests and renew our collective energies by commensality and cobibiality.

As cultists, we have our own codes; but like all codes, our own also contain ambiguities. Some of these ambiguities, I shall suggest, stem from our ambiguous position in the world. I am assuredly not the first to announce this fact; but I shall devote this paper to unraveling some of these ambiguities and to pointing to resultant paradoxes which, I believe, threaten the continued productivity and creativity of anthropology.

Some of the ambiguities arise from our origins. Before we moved off to the peripheries of the world, to live among "savages" and "barbarians", we were first engaged in the search for our own primitive and barbaric past; we were folklorists and ethnomusicologists before we became

ethnologists; and we were narrow nationalists before we became *internationalists*. As protagonists of the 'folk', we participated in the forging of national identities against supernational strata such as the aristocracy and the clergy. This process, though now in abeyance in Western Europe, is still strong in Eastern Europe, where a populist nationalism is asserting itself against foreign elites but recently disestablished (Halpern and Hammel, 1968). By bestowing a high value on folklore – and I am using the term in its broadest sense – we also underwrote the claims of the folk for membership in the larger society. We found them valued ancestors, we constructed their cultural genealogies and we extolled their autonomous creativity. Connecting the folk of the present with a putative past, we thus validated their credentials for entry into the wider national arena. Yet our position was ambiguous even then; supporting the social and political mobilization of the folk against the top estates, we also created for them a fictitious history, based on a nostaligia of the past, defended against the new values of the bourgeoisie of the city and its proletariat. Using for his symbol the conservative Cato the Elder, Barrington Moore has recently characterized this stance as 'Catonism' and has noted that wherever 'commercial relationships have begun to undermine a peasant economy, the conservative elements in society are likely to generate a rhetoric of extolling the peasant as the backbone of society' (1966, p. 491). And he goes on to characterize this feudal rhetoric as follows:

> The organic life of the countryside is supposedly superior to the atomised and disintegrating world of modern science and modern urban civilization. The peasant's alleged attachment to the soil becomes the subject of much praise and little action. Traditional religious piety with archaising overtones becomes fashionable. Actually, as in the case of Japanese *Shinto*, the tradition is to a substantial degree cooked to order, though not entirely. Obedience, hierarchy, often with overtones of race or least biological metaphors about society, become the watchword. But the hierarchy is not supposed to take on the character of modern impersonal bureaucracy. Indeed there is much talk of comradeship, human warmth ... the emphasis on human warmth seems to be as decisive an element as the notion of moral regeneration (pp. 492–493).

And he adds wryly that where political and intellectual leaders 'talk mainly about moral virtues ... many poor devils are liable to be badly hurt'.

Here, then, is the first paradox: In validating the folk, we also all too often validated the country squire and the samurai. This gave rise, in

the next phase, to a validation of hierarchical rule, when the submerged folk of yesterday began to establish new claims as universal colonizing elites. If the first step was to assert that we Celts, 'nous aussi, nous avons des aieux', the next step was to interpret history as a dialectic between passive autochthonous populations and active history-making migratory rulers. Thus, many a protoanthropological folklorist became, in the course of the nineteenth century, a full-fledged anthropological racist. Anthropology, then, was already speaking with a 'forked tongue', with two voices.

The anthropological paradox deepened and widened as Europe set out to conquer the world. This movement over the surface of the globe 'accomplished a transformation which created the world as a social system' (Worsley, 1964, p. 14), but it also shattered numerous multilineal growths that had marked the human past and subjected them to the overarching primacy of an order itself dualistic and paradoxical. 'Europe', says the Malagasy poet and leader Rabemananjara,

> had inherited the privilege of Janus; she had two faces. On one side, a face of stone, of death, a grimacing face, Gorgon's face, of unequalled cruelty, cynicism, rascality, and self-satisfaction, the face incarnated by our inventors of Negro barbarism, of the experts in supplying human flesh for the cremation furnace. On the other side, a face of lilies – one kingdom has even made them the symbol of its arms – a face of purity, of spring water and dawn, the marvellous imprint of the mask of Venus, so beautiful that she seems to have embodied in herself the sum of all human perfection, in being the first to call forth from the limbo of our consciousness all the luminous principles of the Rights of Man (1956, p. 27).

It is to our enduring credit, as anthropologists, that in this shattered world we labored so that the past might not be wholly lost. We interviewed the survivors; we recorded native traditions; we studied obscure and disappearing languages; we collected artifacts. Having created, for the folk of our own societies, a nostalgic past in disembodied abstraction from their real fate under the shattering impact of industrial and commercial change; 'we proceeded to collect other peoples' cultures like so many fossils stranded upon the beach of time. But even as beachcombers and ragpickers of tradition, we also participated in the holocaust. To study men, we treated them as objects (Lévi-Strauss); but to study men as objects – from the privileged position of the imperialist observer – was but to deal with them as they were already dealt with in the world of economic and political coercion. As the laborer was treated as a source of impersonal labor power, abstracted from his total humanity, so we,

too, treated men as sources of tradition, abstracted from their evident human predicament. If others alienated men from their land and labor, we alienated them from their sacred messages and treasured artifacts.

Nowhere is this process more evident that in the burgeoning growth of our great museums. During the nineteenth century these grew from the aristocrats' private collections – containing cameos alongside stuffed two-headed calves – into great public treasure houses, symbolizing national power and glory and stocked through systematic theft, ranging from the removal of the Elgin marbles to London to the transportation of the Ife heads to Germany. Thus, when the flood receded, the Trobriand Islanders would find their valued armbands in the British Museum, and the Kwakiutl, their vaunted coppers in Ottawa.

We must thus face the uncomfortable fact (see Leiris, 1950) that our work as anthropologists has gone on largely and primarily under the imperialist umbrella, in what Balandier has referred to as the *colonial situation.* Our studies

> in principle, could not (or should not) ignore such an important fact as colonialism, a phenomenon which has imposed, for a century or more a certain type of evolution on subjugated populations. It seemed impossible not to take into account certain concrete situations in which the recent history of these people evolved. And yet it is only now and then that anthropologists have taken into consideration this specific context inherent in the *colonial situation.* ... On the one hand, we find researchers obsessed with the pursuit of the ethnologically pure, with the unaltered fact miraculously preserved in its primitive state, or else investigators entirely absorbed with theoretical speculations regarding the destiny of civilizations or the origins of society. And on the other hand, we find researchers engaged in numerous practical investigations of very limited scope, satisfied with a comfortable empiricism scarcely surpassing the level of using a technique (1966, p. 35).

The colonial situation has also furnished the prerequisites for that odd stance of the anthropologist who works at once as 'stranger' and 'friend', in Powdermaker's excellent phrase (1966). Occupying a privileged position, he moves toward the informant as friend, eliciting information by eliciting his trust; returning again to his privileged sanctuary, he becomes once more the stranger, building the information obtained into an abstract model, different from that furnished him by the friendly natives. We elicit trust and understanding; we mobilize compassion; but then we caution ourselves against becoming 'involved' and return to safe middle distance. Needless to say, only when the natives are friendly,

only when they are powerless, can I make them 'my' people; field work becomes impossible when they grow hostile and when we cannot return to the sanctuary of our home base. Hence the concern of the profession that the conditions for further field work are being undermined by the current escalation of political pressures (Beals, 1967). Yet these pressures merely render explicit what has hitherto been comfortably implicit: Anthropological field work can function only when the field situation is guaranteed by superior power. Yet the anthropologist acts as if that power did not exist, despite the fact that his very mode of operation is predicated upon it. Throughout his field work, the anthropologist must exercise an unusual and curious control over his emotions, his reason and his drives to action. He must identify but not too much; he must guide his reason to select some aspects of the situation but not others; and he must certainly control any drive to action which would markedly increase the capacity for self-management among his putative friends.

The anthropological approach has sometimes been compared to that of the psychoanalyst. Both approaches involve a controlled movement of observer and subject – toward each other in human sympathy, apart in controlled analysis. Both approaches try to render conscious what has hitherto been buried in the unconscious: The analyst tries to uncover the historical sources of behavior now evident in the patient's behavioral repertoire; the anthropologist, to follow Bateson's phrase, tries to locate the source of messages which come to to the native from sources unbeknownst to him. But there are also differences. The psychoanalyst is not only diagnostician but also therapist: he wants to make ego prevail where superego dominated. He hopes to be able to strengthen the organism's self-control and to reduce the capacity of others–parents and parent surrogates–to interfere with his autonomous processes. But this is precisely where the anthropologist ceases his labor for, in taking the step from observation and diagnosis to therapy and prescription, he encounters what he has so thoroughly kept hidden from himself and others, namely, the facts of power.

But the facts of power demand adaptation. What have been some of the adaptive strategies of anthropologists in the face of power? And what have been the consequences of their strategies for the intellectual operations of our discipline? Borrowing from a terminology used to describe intellectual positions current in the declining phases of the Roman Empire, let me speak of these strategies–somewhat pretentiously–as Augustinian, Pythagorean and Manichaean. Let us accept

these labels *pro tem*, without any expectations that they need survive the duration of this discussion. Let us also realize that all of us play now one, now another, now a mix of these strategies. If I overemphasize their distinctions, it is for purposes of clarification not for purposes of empirical description of anthropological work.

The Augustinian strategies accept the world as it is. Augustine, in the fourth century A.D., 'denounced as "a ridiculous fable" the expectation that a Second Coming would transfigure the structure of history; there will be no Paradise on earth and the tension between the two kingdoms will remain permanent. There will be no divinization of society beyond the presence of Christ in his Church' (Molnar, 1961, p. 335). Applied to our present situation, this means that we hold any ultimate values we may own in abeyance, proceed with our work under the umbrella of dominant power and accept as well the continuing limitations on self-regulation in the societies we study. We then have two choices: To identify with power or with the powerless. Some anthropologists have always lent their skills and talents to the implementation of dominance, whether under the incarnation of the Colonial Office, the Bureaux Arabes or our own Invisible Government. Emotionally, such cooperation requires identification with the aggressor; intellectually, it makes the anthropologist an outsider, all stranger and no friend. As Augustinians, however, we may also identify with the powerless. In so doing, we accept such dominance as exists but protest its excesses, whether in the Kalahari or in Vietnam. Emotionally, we identify with the victim; intellectually, however, we curb our use of reason by treating excess as perversions of good rather than as structural consequences of social systems. Paradoxically, this means that we identify with the aggressor anyway since we deny ourselves an understanding of his methods and the sources of his methods. Both Augustinian variants, moreover, limit action. Using the first variant, one labors to adjust the native culture to the facts of power, often under the pretense of saving it. Using the second variant, one binds up the wounds of the powerless, under the pretense that a chorus of lamentation substitutes for an autonomy which would allow the victim to increase his dominion over himself and his condition.

A second set of strategies I would call Pythagorean because–like the Pythagoreans of the latter days of the Roman empire–they stage an intellectual escape from the prison of the real world into the untrammelled realm of the mind, where all is duality and opposition, scales

and numbers, revolving in an eternal round to the accompaniment of the music of the spheres. Here we exchange the kingdom of necessity for an unlimited world of freedom created by the human mind. I discern this strategy in Lévi-Strauss, tilting his magnificent magical kaleidoscope. Perhaps the same drive is present in our componential analysts. An earlier form of this strategy was practiced by the diffusionists, this time with the kaleidoscope reversed, disassembling and reassembling bits of culture according to a magical numerology. This strategy liberates reason equally from the promptings of desire and from the pull of action; but I fear that it renders anthropology trivial because irrelevant to the world.

A third set of strategies hopes to 'salvage history'. In contrast to the Augustinian and Pythagorean strategies, which both accept the world by either submitting to it or fleeing from it, the Manichaean strategies aim to take a historical leap from present evil into utopia. One kind of utopia envisions the *Ordensstaat*, an empire of a thousand years synchronized through the use of overwhelming power. The opposite utopia, that of the oppressed, envisions a world purged of oppression through the extrusion of the oppressors. Both call to action and to that extent enlist both reason and emotion; but to the extent that they focus reason on the technology of achieving and maintaining power and veil analytic reason in emotion-packed myth they render anthropology unnecessary.

Thus anthropology would seem to confront a choice between curtailment, triviality or disappearance. Can it survive? It can quite probably survive, as we are now doing, by alternating among strategies. After all, this has always been the intellectual attitude, and it saved people even in the Nazi concentration camps; one strives to remain cool and collected, to reduce one's internal commitments to the world and to exercise one's analytic reason as best one can. Thus anthropologists could survive much as the monks of the Middle Ages did, together with their tattered manuscripts.

But perhaps the question is not whether anthropologists survive, but whether the contribution of anthropology survives and whether it can surmount the curtailment and trivialization of intelligence. I do not know the answer to this, but I think that – whatever else anthropologists may also do – anthropology needs to discover history, not the ahistorical history of the diffusionists nor the 'one-damned-thing-after-another' school of professional historians, but a history which would account

for the ways in which the modern world was 'created as a social system' and for the implications of this process. Such history would therefore have to be 'critical' history; it would strive to make analytic sense of all societies, including our own. This is a task, assuredly, not simply for anthropologists but for anthropologists together with others, organized into new cults, with new ancestors and a new communion. The alternative, – a continued descent into professional irrelevance and autism.

References

Balandier, George (1966), The colonial situation: A theoretical approach. In Immanuel Wallerstein (Ed.), *Social change: The colonial situation.* New York, John Wiley and Sons. Pp. 34–61.

Barnhart, C. L. *et al.* (1958), *The American college dictionary.* New York, Random House.

Beals, Ralph (1967), International research problems in anthropology: A report from the U.S.A., *Current anthropology,* 8: 470–475.

Halpern, Joel, and Hammel, Eugene (1968), Anthropology in Eastern Europe. Manuscript.

Leiris, Michel (1950), L'Ethnographe devant le colonialisme, *Temps Modernes,* 6 (58): 357–374.

Molnar, Thomas (1961), *The decline of the intellectual.* Cleveland, Meridian Books.

Moore, Barrington (1966), *Social origins of dictatorship and democracy: Lord and peasant in the making of the modern world.* Boston, Beacon Press.

Powdermaker, Hortense (1966), *Stranger and friend: The way of an anthropologist.* New York, Norton.

Rabemananjara, Jacques (1956), L'Europe et nous, *Présence Africaine,* 8, 9, 10: 20–28.

Worsley, Peter (1964), *The third world.* London, Widenfeld and Nicolson.